SEP 25 1951

SER. II, VOL. 13 *May, 1951* No. 7

TRANSACTIONS
OF
THE NEW YORK ACADEMY OF SCIENCES

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TRANSACTIONS of The New York Academy of Sciences, Series II, Volume 13, No. 7, May, 1951.
Entered as second-class matter December 12, 1947, at the post office at Baltimore, Maryland, under the act of August 24, 1912
This publication is distributed to members and is published monthly from November to June, inclusive, by The New York Academy of Sciences, Mt. Royal and Guilford Aves., Baltimore 2, Md. Editorial office: 2 East Sixty-third Street, New York 21, N Y

Executive Director: EUNICE THOMAS MINER

UNIVERSITY OF MICHIGAN

*SECTION OF BIOLOGY**

SYMPOSIUM†

RECENT DEVELOPMENTS IN ENDOCRINOLOGY

Chairman: E. D. GOLDSMITH

The Endocrine Mechanism Controlling the Mobilization of Depot Fat. *By* LOUIS LEVIN AND RITA K. FARBER

Recent Studies on Adrenal Hyperplasia. *By* JOSEPH W. JAILER

The Influence of Adrenal Cortical Secretion on the Metabolism of Normal and Malignant Lymphoid Tissue. *By* ABRAHAM WHITE

THE ENDOCRINE MECHANISM CONTROLLING THE MOBILIZATION OF DEPOT FAT

By LOUIS LEVIN‡ AND RITA K. FARBER‡

Since it has been shown[1] that fat stored in the depots is not static but, indeed, has a substantial turnover rate, it may be surmised that a mechanism exists whereby the organism can and does release depot fat when this is desirable for calorigenic purposes. The factors involved in the mechanism whereby the depot fat is released for transport to the liver and thus becomes available for metabolic conversion are not clearly understood. A clue was provided, however, by the demonstration[2] that appropriate anterior pituitary extracts (APE) are able to produce ketosis when administered to laboratory animals. It has also been shown[3, 4] that appropriate APE causes a rapid accumulation of fat in the liver and, furthermore,[5, 6] that this fat originates from the fatty depots. However, the identity of the responsible anterior pituitary factor has not been established.

Experiments in this laboratory have shown[7] that physical stress (subjection to cold or to strenuous exercise) which induces a requirement for greater than normal energy production causes a marked migration of fat into the liver. This may be largely prevented by providing exogenous calorigenic substances such as glucose. Other stresses, such as exposure to high temperature or low pressure, do not induce fat mobilization. We have further found that not all active ACTH preparations are effective in causing fat mobilization and, even when they are effective, their activity, in so far as fat mobilization is concerned, is very low in relation to the ACTH activity as determined by conventional methods, such as the adrenal ascorbic acid depletion technique. Also, the adrenocortical steroids (cortisone, 17-hydroxycorticosterone, 11-dehydrocorticosterone, 11-desoxycorticosterone) are of themselves unable to cause release of depot fat in either intact or adrenalectomized mice,[8] although in the latter such treatment prevents the usual post-adrenalectomy decrease in liver fat content.

These results all strongly imply that excitation of the pituitary-adrenocortical system is not the key factor in actuating the fat mobilizing reaction and, furthermore, that adrenocorticotrophic hormone is not of itself the "fat-mobilizing" hormone. However, the demonstration[8, 9] that adrenalectomy completely prevents the fat mobilization which otherwise follows fasting, APE administration, or exposure to appropriate stress suggests that the pituitary-

* The Section of Geology and Mineralogy held a meeting on April 2 at which a paper entitled "Opinions on Deformation of the Earth's Crust" was read by H. H. Hess. No abstract of this paper has been received.

† Held at the meeting of the Section, April 9, 1951.

‡ Department of Anatomy, College of Physicians and Surgeons, Columbia University, New York, N. Y.

adrenocortical system is in some way involved, even though it is not the prime factor.

Neither growth, gonadotrophic, nor luteotrophic hormones have been effective[10] in causing an accumulation of fat in the liver in our experiments, nor does inactivation of the thyroid by feeding thiouracil[8] appear to interfere seriously with the fat-mobilizing reaction.

It is, therefore, apparent that two hormonal factors are involved in the fat-mobilizing mechanism. These are (a) the pituitary hormone which triggers the reaction and (b) the adrenocortical contribution without which the triggering hormone is unable to exert its action. To test this hypothesis, a number of experiments were conducted in adrenalectomized mice which were treated with cortisone (1 mg. per day) and, on the last day, with appropriate pituitary extracts. In such cortisone pretreated adrenalectomized mice, excellent mobilization of fat to the liver was elicited[10] by unpurified APE as well as by growth hormone preparations, which were of themselves inactive even in intact animals. It is to be recalled (*vide supra*) that, in our hands, no pituitary extract thus far tested has been effective in otherwise untreated adrenalectomized mice. In additional experiments, growth hormone and certain other pituitary extracts, of themselves inactive, displayed marked activity in intact mice pretreated with cortisone.

These results verify our hypothesis that two hormonal factors are required for mobilization of depot fat to the liver. These are (a) the triggering anterior pituitary substance and (b) adequate (greater than normally supplied by the adrenal cortex) supplies of circulating adrenocortical hormone. Neither is by itself able to elicit the reaction. The triggering hormone is certainly not ACTH. Whether it is identical with growth hormone is not yet established, but it may be stated that some of our available evidence does not agree with this possibility. It becomes obvious, however, that those pituitary preparations which contain the pituitary factor plus ACTH will of themselves be effective in intact mice. In such a case, the ACTH stimulates the adrenal cortex to secrete the large quantity of adrenocortical hormone, probably the 11-oxygenated variety, which is required to precondition the organism so that the triggering factor can exert its action. Other preparations, rich in ACTH but not containing the triggering factor, will not be active. On the other hand, with those preparations (such as the better growth hormone) which contain the triggering factor but are not contaminated with ACTH, a positive response will not be elicited unless exogenous adrenocortical hormone is administered. Since a positive response is readily obtained in adrenalectomized animals pretreated with cortisone, it is evident that the action of the triggering factor is not mediated by the adrenal cortex but rather exerts a direct action, presumably on the enzyme systems of the fatty depots.

On the basis of the above, one may attempt to reconstruct the events which occur in an animal subjected to fasting or to a calorie-requiring stress. It is visualized that such treatment results in the secretion of two pituitary hormones, *i.e.*, ACTH and the triggering factor. The former stimulates the adrenal cortices to secrete increased amounts of 11-oxygenated steroids, and these in turn condition the organism in an as yet unknown fashion, thus rendering it responsive to the triggering factor, possibly growth hormone, which sets off the actual release of fat from the depots.

Acknowledgments. The research on which this paper is based was supported by a grant (RG1474c) from the Division of Grants and Fellowships of the U. S. Public Health Service.

The senior author (L. L.) was able to engage in the reported investigations by virtue of permission from the Office of Naval Research to allocate a portion of his time to laboratory research.

Many of the growth and adrenocortico-

trophic preparations used in the reported work were furnished by The Armour Laboratories through the courtesy of Dr. Irby Bunding and Dr. J. R. Mote. A portion of the cortisone used was furnished by Merck and Company through the courtesy of Dr. J. M. Carlisle.

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RECENT STUDIES ON ADRENAL HYPERPLASIA*

By JOSEPH W. JAILER†

Wilkins, Fleischman, and Howard (1940) first characterized the syndrome of congenital adrenal hyperplasia with Addisonian-like symptoms. This disease is a self-contradiction in that, although the infants present the symptoms of sodium deficiency, the adrenals are hypertrophied and are secreting excessive amounts of androgens.

Three infants with this syndrome were studied recently in an attempt to investigate further the underlying physiological pathology. These patients were first seen at a very early age, having failed to regain their birth weights. On admission, they were definitely dehydrated, and the serum sodium was extremely low, while the K and NPN were elevated. It was finally necessary to administer desoxycorticosterone acetate (DCA) in addition to supplementing the diet with 5 gm. of added salt, in order to maintain serum sodium values at approximately normal levels. As far as could be determined, however, there were no abnormalities in carbohydrate metabolism. A diagnosis of adrenal insufficiency was made on the first admission of all three patients.

One was admitted again at 15 months of age and it was noticed at this time that his penis was larger than normal. X-rays of the skeleton revealed an advanced maturation of the ossification centers. The urinary excretion of neutral 17-ketosteroids averaged 10 mg./day, or about 10 times normal. The values for formaldehyde liberating steroids (F.S.L.), however, were within normal limits (0.2 mg.) for a boy of his age.

Thus, from the physiological point of view, it would appear that his adrenals were secreting a normal quantity of 11 oxycorticosteroids, an excessive amount of androgens, and deficient amounts of desoxycorticosterone-like (DCA) hormones. Investigation of the adrenal area by peri-renal insufflation revealed bilateral adrenal hyperplasia.

Lewis and Wilkins (1949) have postulated that the underlying pathology in this condition is not a deficiency in DCA-like hormones as originally thought but rather an oversecretion of a DCA-like antagonist which causes the renal excretion of sodium rather than retention of this electrolyte. However, the evidence presented was quite equivocal.

When the patient described in detail was admitted recently for a minor upper respiratory infection, it was decided to investigate the problem to ascertain whether the hypothesis of Wilkins could be confirmed.

L. Y. was 2½ years old at the onset of this experiment. He was placed on the hospital salt-free diet to which were added 5 gm. of supplemental sodium chloride. Five mg. DCA were injected daily throughout the experiment.

During the control period, the average

* Aided by grants from the American Cancer Society recommended by the National Research Council, Committee on Growth, and the Ciba Pharmaceutical Company.
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excretion of sodium was 68 meq./day and, after the administration of ACTH, there was a gradual rise in sodium excretion until it reached 132 meq./day. There was a fall of almost 10 meq. in the serum sodium. With this, the patient lost 400 gm. and looked poorly and his blood pressure fell to 90/60. That his adrenals had been stimulated is evidenced by the rise in 17-ketosteroids and corticoids (F.L.S.) (TABLE 1). These data suggest that stimulation of these adrenals causes a diuresis of sodium rather than a retention (Thorn, Sprague). These would tend to confirm the results of Wilkins, *et al.* (1949).

TABLE 1

THE EFFECT OF ACTH AND CORTISONE ON A PATIENT WITH "ADRENAL HYPERPLASIA ASSOCIATED WITH ADDISONIAN-LIKE SYMPTOMS"

Day	*Urine Na* meq.	*Serum Na* meq.	*17 K-S* mg./day	*F.L.S.* mg./ day	*Treatment*
1	70.2	132.8	12.2	0.19	DCA
2	66.4		12.5	0.18	DCA
3	86.4		15.1		DCA ACTH
4	127.2		21.1	0.92	DCA ACTH
5	109.0	124.0	20.4	0.94	DCA ACTH
6	132.6		22.9	0.62	DCA
1	56.2		9.6	0.28	DCA
2	47.3*	139.0	7.2	0.16	DCA
3	83.4		11.6		DCA
4	41.7	139.5	2.7	0.80	DCA cortisone
5	66.0		3.5	0.31	DCA cortisone
6	50.4	142.5	5.0		DCA cortisone

* Some urine was lost on this day.

The administration of 100 mg. a day of cortisone, subsequently, resulted in no significant change in urinary sodium and an equivocal rise in serum sodium (TABLE 1). Of further interest is the result that the urinary 17-ketosteroid excretion fell to approximately normal values on this regimen. Previously, withdrawal of DCA resulted in a fall in serum sodium, but on daily cortisone it was possible to stop DCA therapy and still maintain serum sodium levels at normal levels indefinitely.

It is speculated that the adrenals in this syndrome are secreting an abnormal steroid whose principal physiological action is one of sodium excretion rather than retention. (It may be a known steroid, since most of the steroids isolated in the urine have not been tested for physiological activity.) Stimulation of these adrenals would therefore enhance sodium excretion. On the other hand, cortisone probably inhibits ACTH secretion by the pituitary and causes a temporary atrophy of the adrenals and, at the same time, acts as replacement therapy. Thus, since the secretion of this abnormal steroid is suppressed, it is no longer necessary to supply exogenous DCA.

TABLE 2

THE EFFECT OF CORTISONE ON THE URINARY EXCRETION OF 17 KETOSTEROIDS IN PATIENTS WITH ADRENAL HYPERPLASIA

C.D.		*W.H.*	
Day	*17 K.S. mg./day*	*Day*	*17 K.S. mg./day*
1	8.9	1	20.0
2	14.1	2	21.6
3	16.5	3	17.6
Cortisone 50 mg./day		*Cortisone 100 mg./day*	
2	4.7	3	2.4

Three other patients with the pure adreno-genital syndrome (without electrolyte disturbances) due to adrenal hyperplasia have been given cortisone, and in each there has been an immediate fall in the urinary 17-ketosteroid excretion to approximately normal values. Two of these are presented in TABLE 2. These data would confirm the hypothesis that cortisone inhibits the secretion of pituitary ACTH.

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THE INFLUENCE OF ADRENAL CORTICAL SECRETION ON THE METABOLISM OF NORMAL AND MALIGNANT LYMPHOID TISSUE*

By ABRAHAM WHITE†

In 1945, Dougherty and White[1] described in detail the histological changes occurring in lymphoid tissues of mice and rabbits as a result of a single, subcutaneous injection of either adrenal cortical extract, adrenal cortical steroids oxygenated in position 11, or pituitary adrenocorticotrophic hormone. As a working hypothesis, it seemed reasonable to assume that these histological alterations, characterized by lymphocyte dissolution, pycnosis, and karyhorrexis, were accompanied by biochemical alterations in the metabolism of the lymphocyte. There was initiated, therefore, a program designed to examine and to compare specific biochemical processes in lymphocytes of control, adrenalectomized, and adrenal cortical extract- or ACTH-treated animals. Moreover, it was thought that data of this type would afford a basis for examining the influence of pituitary-adrenal cortical secretion on malignant lymphocytes. The data being presented were obtained at the University of California at Los Angeles, in collaboration with Dr. Sidney Roberts.

An approach to the problem of the mechanism of a possible effect of pituitary-adrenal cortical secretion on lymphocyte functioning was sought by studies of the metabolism of lymphoid tissue *in vitro*. The experimental method may be described briefly as follows. Thymus, mesenteric lymph nodes, and spleen were obtained at autopsy from male, adult, Sprague-Dawley rats. Liver was studied for comparative purposes. Minces of each of these tissues were incubated with shaking at 38°C. in Warburg type flasks in serum obtained by bleeding rats under light ether anesthesia from the abdominal aorta. The period of incubation varied from one to twelve hours, although in most experiments a three-hour period was employed. Conditions were either aerobic (95 per cent oxygen-5 per cent carbon dioxide) or anaerobic (nitrogen). At the end of the incubation period, the incubation mixture was centrifuged, and analyses were conducted on the serum and occasionally also on extracts of the tissue residue. These extracts were prepared by first washing the residue thoroughly three times with physiological saline and then extracting by grinding with a small amount of sand and physiological saline.

The rate of release *in vitro* of protein nitrogen from minces of spleen or of liver was found[2] to be affected by the level of adrenal cortical functioning in the rats from which these tissues were obtained. This rate was lower in tissues from adrenalectomized animals and higher in adrenal cortical extract-treated rats, as compared to controls. The nature of the protein alterations was studied[2] by electrophoretic examination of the changes occurring in the serum used as the incubation medium. The data reveal that liver or splenic minces from normal or adrenalectomized rats removed albumin from the medium and, in the case of the liver, added to the serum a protein with an electrophoretic mobility characteristic of the alpha globulins, while spleen released a protein with an electrophoretic mobility of the beta globulin of rat serum.

Support of the observations with liver tissue was sought in the partially hepatectomized rat.[3] In this experimental subject, the rapid fall in the total serum proteins was even more rapid if the animals were previously adrenalectomized, and was almost wholly prevented in the rats which received daily injections of ACTH for four days prior to partial hepatectomy. These alterations in total serum proteins were in good correlation with the nitrogen content of the livers of animals from the respective groups.

As in the case of normal lymphoid tissue, the rate of protein release *in vitro* from malignant lymphoid tissue appeared to be

* The data discussed in this paper were obtained in investigations conducted with the aid of a grant from the American Cancer Society (on recommendation of the Committee on Growth of the National Research Council).

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affected by the level of adrenal cortical secretion in the rats in which the transplantable lymphosarcoma had been growing.[4] Electrophoretic examination of the serum medium prior to, and following, the incubation period revealed an addition to the beta globulin component, with a slight decrease in the albumin fraction. It is of some interest that most of the sera obtained from rats bearing the transplantable lymphosarcoma had an elevated beta globulin component.

Two additional metabolic phenomena of lymphoid tissue *in vitro* have been found recently[4] to be influenced in rate by the level of adrenal cortical activity in the rats from which the lymphoid tissue is taken. The extent of release of amino acids from *normal* lymphoid tissue minces is the same when comparisons are made of mesenteric lymph nodes from control, adrenalectomized, and adrenal cortical extract-treated rats. On the other hand, a higher rate of amino acid release is found with minces of transplantable lymphosarcoma, as compared to the normal lymphoid tissue, and is depressed to the level of that obtained for normal lymphoid tissue if the lymphosarcoma-bearing rat is pretreated with adrenal cortical extract. Furthermore, while the rate of aerobic glycolysis in normal lymphoid tissue is unaffected by either previous adrenalectomy or adrenal cortical extract treatment, there appears to be a stimulatory effect of previous adrenal cortical extract injections on the already elevated aerobic glycolysis of malignant lymphoid tissue *in vitro*.

These studies of the influence of pituitary-adrenal cortical secretion on the metabolism of lymphoid tissue *in vitro* are undoubtedly still rather remotely removed from providing an answer to the question of the precise reactions in lymphocytes which are affected by adrenal cortical hormones. Indeed, no conclusive evidence has been provided, as yet, that the adrenal cortical steroids exert a direct influence on lymphocytes *in vivo*, although there are a few *in vitro* data which are in agreement with a direct action. Nevertheless, the data which have been described in this presentation afford additional support for the now accepted concept[5] of a regulatory influence of pituitary-adrenal cortical secretion on the structure and function of both normal and malignant lymphocytes. Further dissection of the mechanisms by which this regulatory influence functions may be achieved by the *in vitro* approach which has been described.

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THEORY OF MOVING CONCENTRATION BOUNDARIES*

By WALTER H. STOCKMAYER†

If a boundary is formed in an electrolytic cell between two solutions of the same salt, as for instance NaCl (c'): NaCl (c''), it will generally move when an electric current is passed through the cell. A simple analysis[1] shows that, on the passage of N faradays of electricity, the volume ΔV swept out by the boundary is given by

$$\Delta V/N = (T' - T'')/(c' - c''), \quad (1)$$

where T', T'' are the transference numbers of the cation constituent in the two solutions and c', c'' are the corresponding concentrations in equivalents per unit volume. Thus, a measurement of the motion of such a "concentration boundary" offers in principle a means of obtaining directly the transference-number difference $(T' - T'')$. This possibility, though recognized by Planck and Kohlrausch before 1900 and semiquantitatively confirmed by E. R. Smith in 1931, was developed into a precise experimental realization by Longsworth[2] only a few years ago.

Compared to ordinary moving boundaries involving two different electrolytes, such as LiCl (c'): KCl (c''), concentration boundaries are characterized by a much slower rate of movement and a far greater tendency to spread by diffusion. If disturbances due to convection are to be avoided, the current must be kept at so low a value that the boundary has moved only a very short distance before it becomes too obscure for simple visual observation. Longsworth's development of the schlieren scanning method,[3] however, made possible the accurate observation of the diffuse boundaries thus generated.

The purpose of this note is to present briefly a partial solution to the mathematical problem encountered when an attempt is made to predict in detail the spreading of such a boundary with the passage of time; that is, to represent mathematically the salt concentration at each point in the vessel as a function of time. Aside from the intrinsic interest in having a complete theory for such boundaries, the solution of the problem has a degree of practical merit in that the *curvature* d^2T/dc^2 of the transference-number function can, with its aid, be computed from experimental observations of the boundary, thus aiding materially in the smoothing of transference data.

The partial differential equation governing the salt concentration c (in a tube of uniform cross-section through which a current flows axially) is

$$\frac{\partial c}{\partial t} = \frac{\partial}{\partial h}\left(D\frac{\partial c}{\partial h}\right) - \frac{i}{F}\left(\frac{dT}{dc}\right)\frac{\partial c}{\partial h}, \quad (2)$$

where t is time, h distance along the tube axis, D diffusion-constant, i current density, and F the faraday. It is interesting to remark that this relation, derived by Longsworth[2] on the classical but slightly incorrect assumption that the mean force on an ion depends only on the electrical field-strength and the gradient of its own chemical potential (in other words, that electrophoretic mobility is identical with diffusion mobility), can in fact be derived unchanged from the more general laws given by Onsager and Fuoss.[4] The effect of the current is clearly contained in the last term of equation 2. If this vanishes, the law of free diffusion alone is recovered.

A general solution of equation 2 is not possible at present, so a tractable form for dT/dc must be introduced. If the transference number can be fitted by a quadratic over the concentration interval from c' to c'',

$$T = a_0 + a_1c + a_2c^2,$$
$$dT/dc = a_1 + 2a_2c; \qquad c'' \leq c \leq c', \quad (3)$$

* This paper, illustrated with lantern slides, was presented at the meeting of the Section, April 10, 1951.

† Department of Chemistry, Massachusetts Institute of Technology, Cambridge, Massachusetts.

a mathematical discussion is feasible. As shown by Longsworth,[2] if the curvature a_2 is zero, so that dT/dc is constant, the spreading of the boundary will be the same as if no current were flowing. If a_2 does not vanish, however, the boundary will spread either more or less rapidly than in the case of free diffusion. If it spreads *less* rapidly, the boundary will eventually attain a *steady state*, after which it will still move at constant speed but will no longer continue to spread. Longsworth actually observed the steady state for boundaries of cadmium iodide, for which a_2 is unusually large, but in general this state cannot be realized in practice. The tendency in this direction is clearly visible from the experimental results, however, one of the two boundaries in the Tiselius-type cell always spreading less rapidly than the other. To make use of these observations, a more detailed solution of equations 2 and 3 is needed.

If we make the substitutions

$$u = (2c - c' - c'')/(c' - c''), \quad (4a)$$
$$v = i(T' - T'')/F(c' - c''), \quad (4b)$$
$$x = h - vt, \quad (4c)$$
$$\lambda = ia_2(c' - c'')/F, \quad (4d)$$

and also assume that D is constant, the differential equation becomes

$$\frac{\partial u}{\partial t} + \lambda u \frac{\partial u}{\partial x} = D \frac{\partial^2 u}{\partial x^2}. \quad (5)$$

The quantity v in equation 4b will be recognized as simply the linear velocity of the boundary (compare equation 1). Thus, by changing from the coordinate h to the new coordinate x of equation 4c, we cause our origin to "move with the boundary," so that equation 5 no longer is concerned with the speed of the boundary, but only with its shape.

The above differential equation also appears in other physical problems. It describes approximately the flow through a shock wave in a viscous compressible fluid, in which case u represents the excess of flow velocity over sonic velocity, D is proportional to kinematic viscosity, and λ is related to the adiabatic compressibility. It has also been used by J. M. Burgers as a simple mathematical model for turbulent flow. These applications of the equation, as well as its general solution, are discussed fully in a current paper by Cole.* It is there shown that the substitution

$$u(x, t) = -\frac{2D}{\lambda} \frac{\partial \ln \theta}{\partial x} \quad (6)$$

leads to the linear diffusion equation

$$\frac{\partial \theta}{\partial t} = D \frac{\partial^2 \theta}{\partial x^2} \quad (7)$$

for the new dependent variable $\theta(x, t)$. Since we consider the boundary to be contained in a very long tube, the desired solution of equation 7 is that appropriate to an infinite range of the variable x, which is

$$\theta(x, t) = (4\pi Dt)^{-1/2} \int_{-\infty}^{+\infty} e^{-(x-\xi)^2/4Dt} \theta_0(\xi)\, d\xi, \quad (8)$$

where $\theta_0(x)$ is the value of θ at the initial time $t = 0$. Returning to the physically interesting quantity u by means of equation 6, we obtain

$$u(x, t) = \frac{\int_{-\infty}^{+\infty} e^{-(x-\xi)^2/4Dt} e^{-(\lambda/2D)\int_0^\xi u_0(\eta)d\eta} u_0(\xi)\, d\xi}{\int_{-\infty}^{+\infty} e^{-(x-\xi)^2/4Dt} e^{-(\lambda/2D)\int_0^\xi u_0(\eta)d\eta} d\xi} \quad (9)$$

where $u_0(x)$ is the initial value of u, describing the distribution of concentrations through the boundary at the moment the current is started.

If the boundary is perfectly sharp when the current starts, the integrals presented by equation 9 can be evaluated in terms of well-known functions. For this case, the initial conditions may be written

$$u_0(x) = +1 \text{ for } x < 0, \quad = -1 \text{ for } x > 0, \quad (10)$$

* The writer is grateful to Dr. J. D. Cole (California Institute of Technology) for an advance copy of his manuscript, which is to appear in Quarterly of Applied Mathematics.

which corresponds to having the solution designated by a single prime at the left (compare equation 4a). The solution to equation 9, most compactly expressed in terms of the dimensionless variables

$$x_r = \lambda x/4D, \qquad t_r = \lambda^2 t/4D, \tag{11}$$

then is found to be

$$-u(x, t) = \frac{2\sinh 2x_r + e^{2x_r} erf\,(x_r t_r^{-1/2} + t_r^{1/2}) + e^{-2x_r} erf\,(x_r t_r^{-1/2} - t_r^{1/2})}{2\cosh 2x_r + e^{2x_r} erf\,(x_r t_r^{-1/2} + t_r^{1/2}) - e^{-2x_r} erf\,(x_r t_r^{-1/2} - t_r^{1/2})}. \tag{12}$$

For positive values of λ, this equation describes the boundary which spreads less rapidly than a freely diffusing one and

TABLE 1
CURVATURE OF CATION TRANSFERENCE NUMBER WITH CONCENTRATION

Boundary	a_2 *(from observations of boundary shape)*	a_2 *(from tabulated transference numbers)*
0.1–0.2N $LaCl_3$	2.9×10^5	3.0×10^5
0.2–0.4N $LaCl_3$	1.0×10^5	1.0×10^5
0.2–0.5N LiCl	3.1×10^4	2.9×10^4

which eventually reaches a steady state. Thus, when the time increases indefinitely, equation 12 reduces to

$$-u(x, \infty) = \tanh(\lambda x/2D), \tag{13}$$

which is the steady-state result already given by Longsworth.[2] For negative values of λ, the equation describes a boundary spreading more rapidly than by free diffusion; and a zero value of λ, of course, corresponds to the case of free diffusion.

Unfortunately, it was not convenient in Longsworth's experiments to start passing the current when the boundary was sharp. Rather, the boundary was first allowed to spread by free diffusion for a time t_0. The initial condition then becomes

$$u_0(x) = -\mathrm{erf}(x/2\sqrt{Dt_0}), \tag{14}$$

but now equation 9 no longer yields a simple analytical result. Therefore, to obtain most economically a means of evaluating the experimental results, the following more limited procedure has been used.

If the transference-number function is accurately given by equation 3, it can be shown that the boundary is always symmetrical about the origin $x = 0$, the maximum concentration gradient occurring there. For analyses based on this equation, therefore, it is sufficient to calculate values of the derivative $(\partial u/\partial x)_0$ at the origin as a function of time. From equation 9, this quantity is found to be

$$\left(\frac{\partial u}{\partial x}\right)_0 = \frac{1}{2Dt} \frac{\int_0^\infty e^{-\xi^2/4Dt} e^{-(\lambda/2D)\int_0^\xi u_0(\eta)d\eta} \xi u_0(\xi)\, d\xi}{\int_0^\infty e^{-\xi^2/4Dt} e^{-(\lambda/2D)\int_0^\xi u_0(\eta)d\eta}\, d\xi}, \tag{15}$$

where u_0 is to be obtained from equation 14. We have fitted empirical expressions to the integrands appearing in equation 15, expressing these in terms of exponential functions which yield to analytical treatment. The small differences between these empirical expressions and the correct integrands can be integrated graphically. Details need not be given here. The result is that curves of $(\partial u/\partial x)_0$ as a function of the "reduced time" t_r of equation 11 can be constructed for various assigned values of the reduced time of free diffusion, $(t_r)_0$, corresponding to the t_0 of equation 14. By fitting the observed values of $(\partial u/\partial x)_0$ in a given experiment to one of this family of theoretical curves, the value of the parameter λ/D for the boundary can be determined.

Thus far, only limited calculations have been completed, for the sharper boundary alone. These have been sufficient, however, to show that the theoretical function is capable of reproducing the experimental curve for a given boundary rather well, and hence that consistent values of the curvature, $2a_2 = d^2T/dc^2$, can be deduced. The

results for three boundaries studied by Longsworth* are briefly summarized in TABLE 1. The values of a_2 given in the second column were obtained, as described above, from the observed variation of $(\partial u/\partial x)_0$ with time for the sharper boundary. The accuracy of these results cannot yet be discussed with certainty, but a ten per cent change in a_2 gives an obviously poorer fit of the experimental results. For comparison with these figures, the third column lists values of a_2 computed by Longsworth from his published values[2] of the transference numbers for these salts. In each case, a quadratic equation was passed through the values of T at c', c'', and one intermediate concentration. It can be seen that the agreement between the two sets of values of a_2 is highly satisfactory. In fact, the differences between them are small enough so that the computed transference numbers over the given concentration interval agree to better than one digit in the fourth decimal place.

Calculations are now being made for the other (rapidly spreading) boundary. When these are completed, it will be possible to obtain more precise values of a_2.

The present investigation does not exhaust the mathematical problem presented by concentration boundaries. Longsworth observed in many cases a definitely skew boundary, so that at least a cubic term in concentration must be added to equation 3. The resulting differential equation seems too formidable for general solution, but it should be possible to derive approximate results by treating the cubic term as a small perturbation.

A full account of these calculations will be given elsewhere. Thanks are due E. F. Casassa for assistance with the numerical work. It is a pleasure to acknowledge the obviously fundamental help of Drs. L. G. Longsworth and J. D. Cole, as well as the interest and encouragement of Dr. D. A. MacInnes.

The writer is indebted to Dr. Longsworth for the use of these unpublished data.

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TWIN STUDIES IN RELATION TO ADJUSTIVE PROBLEMS IN MAN*

By FRANZ J. KALLMANN†

In investigations of the interaction of genetic and environmental factors in the origin of human behavior disorders, one may generally expect to encounter complex problems of causality. Since many variations are known to exist in the individual ability or inability to maintain an adaptive equilibrium under different conditions of stress, the emphasis in studies of this kind should be placed on the collection of longitudinal records of comparable and controlled data on the entire range of variability in the adjustive potentialities of man, rather than on a search for magically simple formulae of omnipotential or unipotential powers.

As a rule, only two avenues of approach are available, if one aims at differentiating between genetic and environmental influences in relation to human adjustment or maladjustment. One approach is based on an analysis of variations displayed by different genotypes in a constant environment, and the other approach is based on a comparison of the effects of environmental variations upon a constant genotype. In reality, of course, neither procedure is applicable without some degree of imperfection. If the first procedure is used, as is done in studies of orphans and foster-children, it is necessary to operate with such approximations as the relatively similar environments provided by institutions or specially selected family units. In an attempt to apply the second method, the main difficulty is posed by the fact that, except for a very small number of one-egg twins, no inbred population of identical genotypes is available in man, comparable to the "pure strains" used in animal and plant genetics.

It is fortunate, therefore, that the phenomenon of twinning supplies us with sets of genotypically identical individuals, whose developmental and adjustive variances can be compared with those observed in the differential behavior patterns of ordinary siblings. Of course, this comparison of intra-pair differences requires long-term observation and adequate sampling procedures, if the histories of the twin subjects used as index cases are supposed to be representative of the total population, from which they have been drawn.

In the organization of twin studies, it is also advisable to be aware of certain methodological limitations. It should be understood, for instance, that the use of this method will not make it possible to assign exact quantitative values to the respective contributions of genetic and environmental factors in the production of individual differences. Evidently, the average difference between one-egg twin partners is not a precise measure of environmentally produced variations, while an increase in the average difference between two-egg twins should not be taken as an indication of the contribution of genetic influences alone.

Another procedural limitation may often be seen in the fact that even one-egg twins reared apart do not give a measure of the maximum effect of environment on a given trait, since their prenatal environment is bound to be similar and such important factors as age and health status of the mother remain constant. On the other hand, two-egg twins are genotypically as alike as brothers and sisters and, therefore, do not represent extreme genetic differences.

Many limitations of the original twin study method can be overcome by a procedural refinement, which extends the num-

* This paper, illustrated with lantern slides, was presented at the meeting of the Section, April 16, 1951. Acknowledgments are made to the Rockefeller Foundation, to the Committee for Research in Problems of Sex, National Research Council, and to the Committee on Dementia Praecox of the Scottish Rite for research aid toward this paper.

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ber of genotypes available for comparison under similar conditions of culture and home milieu. This modified version, called by us the twin family method, combines the study of a random sample of both one-egg and two-egg twin pairs with a study of their full siblings, half-siblings, and step-siblings. In this manner, it has been possible in our investigations to integrate controlled observations obtained with the twin study method into a broad study of twin family units, which are assembled by means of an approach combining the statistical requirements of Weinberg's sampling method, as applied to the study of siblings, with the general principles of a procedure approximating a statewide population census of twins in certain age brackets. The present number of twin index families, kept under observation throughout the State of New York, has reached a total of 5108 twin index units.

In a review of comparative twin data on variable adjustive patterns, observed in this manner, it seems advisable to begin with evidence of genetically controlled variations in the ability to maintain a normal state of health and emotional equilibrium under comparable environmental conditions. It is indicated by many observations in siblings and two-egg twin pairs that similar human traits are not consistently produced by similar environments; nor are consistent dissimilarities produced by clearly dissimilar life histories. In fact, there are some persons who seem capable of adapting themselves to varying combinations of distressing circumstances *without* the development of severe maladjustment. The list of frustrations known to produce no serious disorder in certain people is practically unlimited. In particular, it can easily be shown that similar constellations of unfavorable childhood experiences frequently produce no similar personality deviations in genetically dissimilar members of the same family unit, while genetically similar twin partners often display the same type of maladjustment even if separated at a very early age.

The observation, that certain disorders are precipitated by unfavorable environmental influences in only a limited number of persons, by no means minimizes the psychodynamic significance of these constellational factors in potentially vulnerable individuals. The given limitations would seem to make it rather difficult, however, to insist on considering human adaptability to be so flexible in nature as to justify the principle of explaining any deviation from normal adjustive patterns entirely on the grounds of past emotional experiences, that is, without the assumption of variable individual vulnerabilities. In the last analysis, such codified explanations of maladjustment as the currently rather popular one, ascribing almost every known variety of psychopathology to maternal imperfection during the child's early life, merely serve to affirm some basic biological truisms. No one has ever denied that a person must have a mother, and must pass through infancy, in order to have any earthly chance of demonstrating genetically controlled variations in his capacity for physical adaptation, emotional stability, and social adjustment.

The difference in the degree of similar adjustive potentialities, observed between one-egg and two-egg groups of twins, is found to express itself throughout life and also prevails with respect to significant variations in the capacity for adjustment to aging and old age.[5] As to physical signs of aging, a wide range of variability is the rule in two-egg pairs, even in those who always lived under comparable environmental conditions. By contrast, the adaptive similarities between aging one-egg twins are generally much more pronounced and frequently expressed against the potential effect of modifying influences arising from entirely different environments. These similarities extend to the degree of general enfeeblement or its absence, to the decline in adjustive plasticity, to measurable intellectual functions, and to the capacity for longevity. Our present comparative data on the life spans of those index pairs, of

whom both members died of natural causes in the age group over 60, are summarized in TABLE 1, while the mean intra-pair differences in the test scores observed in a random sample of 240 senescent twin index cases[1] are compared in TABLE 2.

TABLE 1

MEAN INTRA-PAIR DIFFERENCES IN THE LIFE SPAN OF 69 DECEASED TWIN INDEX PAIRS OF THE SAME SEX*

Age group (referring to first-deceased twin partner)	*Mean intra-pair differences in longevity (expressed in months of life)*					
	One-egg pairs			*Two-egg pairs*		
	male	*female*	*total*	*male*	*female*	*total*
60–74	47.1	24.8	39.7	97.9	106.9	101.1
Over 74	36.0	33.6	34.4	38.2	29.2	32.8
All pairs over 60	42.9	31.2	36.7	79.1	63.2	71.8

* Mean intra-pair difference for all opposite-sexed pairs over 60: 126.6 months.

TABLE 2

MEAN INTRA-PAIR DIFFERENCES IN THE TEST SCORES OF SENESCENT TWIN PAIRS

Test	*Male pairs*		*Female pairs*	
	one-egg	*two-egg*	*one-egg*	*two-egg*
Vocabulary	2.47	3.89	2.22	4.77
Digits Forward	1.09	1.05	0.83	1.08
Digits Backward	0.91	0.89	0.73	1.15
Digit Symbol	5.34	6.92	4.42	9.25
Block Designs	3.59	6.88	3.17	6.17
Similarities	2.69	2.95	2.45	4.17
Directions	4.64	6.43	4.92	4.62
Tapping	8.47	18.47	11.65	13.82
Reproduction of Designs	1.82	3.00	2.68	3.90

In relation to the total longevity score after age 60, the average intra-pair difference in monozygotic pairs is approximately one-half that observed in dizygotic pairs, as the total mean difference varies from 36.7 months in the monozygotic group to 71.8 months in the dizygotic group of the same sex, and to 126.6 months in dizygotic pairs of opposite sex. The difference between the two same-sexed groups is statistically significant, and about the same for the two sexes. Biologically, it is to be expected, of course, that this difference falls largely into the age group 60–74, and eventually disappears beyond age 75.

The equally revealing trend in one-egg twins toward a similar intellectual test performance in the period of senescence is demonstrable in all test scores measuring abstract intellectual function, although the difference between the two twin groups is significant at the one per cent level of confidence only for the Vocabulary, Digits Backward, Digit Symbol, Block Designs, and Similarities tests. In addition, it is indicated by some interesting sex differences that certain intellectual changes observable in senescent males precede those shown by aging females. This discrepancy apparently corresponds with general sex differences in longevity and adaptive plasticity.

With respect to clearly pathological forms of maladjustment occurring in earlier stages of maturity, there are also many indications that similar patterns of deviation show no consistent association with similar environments. The evidence is especially plain in regard to successfully executed suicide, a human phenomenon quite commonly suspected of being determined either by an inheritable type of unfitness in the general personality structure or by a special form of psychopathological reaction distinguished by introjective aggressiveness. However, neither assumption is supported by twin observations. In fact, until very recently we had reason to believe that the commitment of suicide was practically the only phenomenon which was unlikely to occur in both members of twin pairs even under similar conditions of maladjustment and privation. This opinion was based on the observation of 27 sets of twin subjects who had remained discordant as to suicide until the end of last year.[4] We concluded, therefore, that the high-tension state apparently required for the release of a suicidal trigger mechanism in a distressed person may be expected to be uniform only as to the finality of its biological effect, but not

in the composition of the varying combinations of motivational factors, which may lead to a self-destructive short-circuit reaction to adverse life conditions.

Toward the end of 1950, we actually succeeded in finding the records of a pair of schizophrenic and overtly homosexual World War II veterans, who had just become concordant as to suicide. They ended their lives at different times (at the ages of 25 and 29, respectively) and in different ways (drowning and gas), and thus became the first set of twins in whom this unusual occurrence has been authentically observed in modern times. The suicides were ascribed to fear of readmission to a mental hospital and served to confirm our previously expressed opinion that "the suicides of two twin partners are apt to occur, but will only be observed by chance," which means that they cannot be considered to be directly related to one another even under similar conditions of unfavorable family background, social frustration, or emotional maladjustment.[6] Therefore, concordant behavior with respect to suicide must be expected to be extremely rare even in one-egg twins.

A completely different situation is encountered in an analysis of twin data on overt homosexual behavior in the adult male.[3] In the group of two-egg twins, over one-half (57.7 per cent) of the cotwins of predominantly or exclusively homosexual subjects yield no evidence of overt homosexuality at any age, and only 11.5 per cent in this sample of cotwins have been given homosexuality ratings 5 or 6 on Kinsey's scheme. By contrast, concordance as to the overt practice and quantitative rating of homosexual behavior after adolescence has been observed in all of our 40 one-egg index pairs. Apparently, only two males who are similar in both the genotypical and the developmental aspects of sexual maturation and personality integration are also apt to be alike in those specific vulnerabilities, which favor a trend toward fixation or regression to immature levels of sexuality. It seems equally significant that most of our index pairs assert to have developed their sexual tendencies independently and often far apart from each other, and that all of them deny categorically any history of mutuality in overt sex relations.

From the standpoint of psychiatric genetics, particular interest is attached to the major psychoses, that is, to the schizophrenic, manic-depressive, and involutional types of severe mental disorder. With respect to these psychoses, it can also be shown that they are not apt to be developed by all the persons in an environment which produced a psychosis in another member of their group. Genetically, there is reason to believe, therefore, that some persons, namely, the carriers of a specific type of predisposition or potential vulnerability, have the biological capacity for reacting to precipitating environmental stimuli with either a schizophrenic or another type of psychosis, while this capacity is not possessed by a number of ordinary people. In other words, the ability to respond with such a severe psychosis cannot be considered to be part of a person's normal biological equipment and, therefore, should not simply be classified as "determined constitutionally," that is, as part of the normal constitution of man or as a potentially "norm-directed" human trait.

In order to substantiate this theory, geneticists have to demonstrate that the tendency to develop a severe psychosis increases in proportion to the degree of blood relationship to a family member showing the given type of psychosis, since organic inheritance cannot operate without the factor of blood relationship. If it is possible to procure adequate evidence for a sliding scale of specific morbidity rates, correlated with the degree of consanguinity, advocates of purely environmental theories should make an attempt to provide plausible explanations for a few plain implications of the genetic hypothesis. One of the inferences is that many members of psychotic index families are expected, in accordance with the laws of statistical probability, to be sufficiently protected against a psychosis-

producing environment by the absence of a specific genetic predisposition, without which a person under stress is assumed genetically to be incapable of reacting with such a psychosis.

The general trend of the comparative data summarized in TABLE 3 is clearly in the direction of specific genetic mechanisms for the different types of psychosis.[2] As a rule, the incidence of schizophrenia is found to be increased in the consanguinity of schizophrenic index cases, and that of manic-depressive psychosis in the consanguinity of manic-depressive index cases. It seems significant, however, that the blood relatives of involutional cases show an increase in both involutional and schizophrenic psychoses, which is paralleled by a moderate increase in involutional and senile psychoses among the blood relatives of schizophrenic cases.

The analysis of age-specific expectancy figures reveals a monozygotic concordance rate of 60.9 per cent with respect to involutional psychosis. This finding indicates that monozygotic twin partners are more likely than dizygotic twins or ordinary siblings (with an expectancy of 6.0 per cent) to be alike in those factors which favor the occurrence of a psychosis in the involutional period. The scheme of interacting causative elements includes progressive impairment of general adaptability, cumulative emotional strain and insecurity due to increasingly conspicuous signs of aging, and the coexistence of certain basic personality traits such as rigidity, compulsiveness, or oversensitivity. The long-range effect of these traits is toward a reduction of the adjustive plasticity of aging persons and, thereby, toward an impairment in their faculty of adaptation to involutional changes. Genetically, it is indicated that the principal causal relationship of that type of emotional instability, which may lead to an involutional psychosis, is to the group of schizoid personality traits and, therefore, indirectly to the schizophrenic disease entity rather than to that of manic-depressive psychosis.

The genotypes underlying the clinically distinguishable symptomatologies of schizophrenic and manic-depressive psychoses, which generally occur before the involutional period of life, have been shown by our comparative twin data to be specific and unexchangeable and apparently to follow a basically single-factor type of inheritance. A comparison of age-corrected expectancy rates reveals that the chance of developing either psychosis increases strictly in proportion to the degree of blood relationship to the respective type of index case (TABLE 3).

TABLE 3

EXPECTANCY OF PSYCHOSES IN PSYCHOTIC TWIN INDEX FAMILIES

Type of psychosis in twin index case	*Expectancy in general population*	*Corrected expectancy rates in relation to original psychosis*				
		parents	*half siblings*	*full siblings*	*dizygotic cotwins*	*monozygotic cotwins*
Schizophrenia	0.9	9.3	7.1	14.2	14.5	86.2
Manic-depressive psychosis	0.4	23.4	16.7	23.0	26.3	95.7
Involutional psychosis	1.0	6.4	4.5	6.0	6.0	60.9

The comparative rates for manic-depressive psychosis vary from 16.7 per cent for the half-siblings of manic-depressive index cases to 95.7 per cent for one-egg cotwins. The observed distribution of the disorder in our twin index families seems best explained by an irregularly dominant mode of inheritance. The affective instability and biochemical dysfunction produced by this genotype have been found to be correlated with a tendency to obesity. If concordant one-egg twin pairs show a definite dissimilarity in the severity of their psychotic symptoms, the display of a milder or more easily controllable manic-depressive syndrome ap-

pears to be associated with a lesser degree of overweight.

The constitutional relationships are different with respect to the variable ability or inability to resist the development, or to counteract the progression, of a schizophrenic psychosis, the main genotype of which is usually transmitted in the collateral rather than in the direct line of descent and, therefore, is believed by us to follow a recessive mode of inheritance. In this group, it is the rule that if one twin remains free of schizophrenic symptoms or shows a milder form of the disease than his cotwin, there is always a difference between the twins in regard to physical development, the difference being consistently in favor of the more resistant twin.

The total expectancy of schizophrenia varies from 7.1 per cent for the half-siblings, through about 14 per cent for the full siblings and dizygotic cotwins, to 86.2 per cent for the monozygotic twin partners. The differences in concordance between one-egg and two-egg groups of twin index pairs show no positive correlation with corresponding degrees of environmental similarity or dissimilarity. About one-quarter of one-egg pairs have been found to develop schizophrenic psychoses in the absence of similar environments. Deficient resistance to the effect of the recessive unit factor for schizophrenia seems to be determined by a nonspecific and certainly multifactorial type of secondary genetic mechanism. Measurable correlates of this mechanism may be seen in the capacity for mobilizing effective mesodermal defense reactions, in the compensatory power of the athletic component of physique, and in the ability to maintain a stabilized level of body weight.

In conclusion, it may be stated that a full understanding of the adjustive problems, discussed in this condensed review, is still a challenging proposition. Further advancement in our knowledge of human adjustment and maladjustment will require the planning of more basic research in relation to the biological components of normal and abnormal patterns of behavior, especially in relation to the intricate interaction of gene-specific biochemical phenomena with general constitutional (adaptive) modifiers and those precipitating outside factors arising from the effect of many uncontrolled imperfections in the structure of modern human societies.

There is reason to believe that carefully conducted twin studies will play a very essential part in this joint progress of human biology and psychology.

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*SECTION OF OCEANOGRAPHY AND METEOROLOGY**

SYMPOSIUM†

THE INFLUENCE OF SOLAR PHENOMENA ON THE WEATHER

Chairman: B. HAURWITZ

Introduction. *By* B. HAURWITZ
Solar Influences on the Weather. *By* H. C. WILLETT
Atmospheric Pressure Changes and Solar Activity. *By* RICHARD A. CRAIG
Possible Effects of Ozonosphere Heating on Sea-Level Pressure. *By* H. WEXLER
Some Notes Concerning the Relationship Between Solar Phenomena and Weather. *By* R. PENNDORF

INTRODUCTION

By B. HAURWITZ‡

It is well known that the solar energy received by the earth has a profound influence on the state and the motion of our atmosphere, and that such variations of the amount of solar energy as occur during the daily and yearly cycle are clearly reflected in the meteorological parameters. Consequently, attempts have been made frequently also to find relations between solar activity and atmospheric variables.

Very pronounced relations have indeed been found between the sun and phenomena occurring in the high atmosphere, such as magnetic storms and radio wave propagation. But the search for connections between solar activity and the lower atmosphere in which the weather phenomena occur has so far failed to yield similarly clearcut results.

Since the radiant energy received from the sun is responsible for the circulation of the atmosphere and its periodic and nonperiodic variations, one has tried to find direct relations between meteorological variables and the incoming solar radiation. Unfortunately, all such determinations of the radiation emitted by the sun are unreliable, because the corrections for atmospheric depletion of solar radiation are rather uncertain. Consequently, this method of approach has not been used to any large extent, and the few indistinct relations which have been found cannot be considered as well established.

Considerably more effort has been expended on the search for relations between weather and sun spots. It is generally recognized today that sun spots, while doubtless a manifestation of solar activity, are not necessarily the best indicator of such types of solar activity as may influence either directly or indirectly the lower atmosphere. Nevertheless, the sun spots have the advantage that they are easily observable, and that they are the only solar phenomenon for which a long series of data is available suitable for statistical investigations. Such studies have revealed certain relations between sun spot numbers on the one hand and meteorological and climatological variables on the other hand, as is discussed in the paper by Dr. Willett and also in a part of Dr. Wexler's paper.

Because of the limitations of sun spots as a measure of solar activity, attempts have been made to find other better indices. With certain types of solar activity, particle emission from the sun is greatly increased. This particle emission manifests itself in geomagnetic perturbations when it reaches the earth's atmosphere. These perturba-

* This Section held a meeting on April 17 at which a paper entitled "Air-Coupled Surface Waves" was read by Frank Press. No Abstract of this paper has been received.
† Held at the meeting of the Section, January 29, 1951.
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tions may, therefore, be taken as indices of solar variations. This procedure is admittedly somewhat indirect, but it has the great advantage that the observations of geomagnetic perturbations extend over a much longer time than those of the areas on the sun from which the particle emission takes place. Dr. Craig's paper deals with the relations which have been found between geomagnetism and the sea level atmospheric pressure. It may be added that similar relations have also been found for pressures at higher levels in the troposphere by Mr. W. Kaciak at New York University.

One of the difficulties in the search for relations between the sun and the lower atmosphere is the lack of an acceptable physical theory which would provide an indication of what to look for. The author proposed therefore, some years ago, a hypothesis according to which the increase in ultraviolet solar radiation accompanying solar flares would heat the ozone layer of the atmosphere to a different extent at different latitudes, and suggested that the circulation which thereby is set up in these layers should make itself felt in the lower atmosphere. It was hoped at the time this hypothesis was suggested that it might serve as a basis for further discussion, even though its details were not worked out and its foundations were admittedly shaky.

The author's hypothesis is examined in the papers by Dr. Wexler and Dr. Penndorf. Dr. Wexler describes his investigations of two atmospheric models which lead him to the conclusion that any effects of enriched ultraviolet radiation on the ozonosphere would not be observable near the earth's surface. In carrying out his mathematical analysis, he considers only the initial and final equilibrium state, because the analysis of any intermediate oscillations which might yield larger effects at the earth's surface would be very complicated. Dr. Penndorf points out that, in connection with an increase in ultraviolet solar radiation, the total amount of ozone should also rise while the ozone layer becomes warmer. Unfortunately, the observation material is still too scarce to conduct reliable statistical checks of the relation between ozone amount and solar parameters. From the available data, however, Dr. Penndorf finds no such relation. Moreover, certain theoretical studies of solar physics indicate that the radiation from the disturbed sun should only increase in certain lines of the spectrum, resulting in a radiation which could not penetrate as far as the ozone layer in the atmosphere. He proposes, therefore, an alternate hypothesis, according to which not the ozonosphere, but the ionosphere is the link between solar and weather variations.

It will be apparent from the foregoing survey and from the following four papers that, as yet, very little is positively known in the whole field, and that much remains to be done before the relationship between the sun and the lower atmosphere and its physical theory are definitely established.

SOLAR INFLUENCES ON THE WEATHER

By H. C. WILLETT*

The purpose of this paper is only to refer very briefly to a few recent studies which point to a direct irregular (nonseasonal and nondiurnal) solar influence on the large-scale weather patterns of the northern hemisphere. It is quite impossible in this paper to do more than report the principal results of only a few recent studies. There can be included no presentation of the complete evidence from which the resultant conclusions are drawn, nor any reference to scores of other studies which may be cited in partial confirmation of some of these conclusions. With the single exception of the study of Duell and Duell and the confirmation by Craig, all of those discussed here were carried out by the Weather Bureau—Massachusetts Institute of Technology Extended Forecasting Project.

The first study to be mentioned is that

* Weather Bureau—Massachusetts Institute of Technology Extended Forecasting Project, Cambridge, Massachusetts.

of Duell and Duell.[1] This work merits particular attention because it is the first study to demonstrate at all convincingly any short-range immediate response of the atmosphere to solar disturbances. The better known part of this study deals with the response of sea level atmospheric pressure in certain areas during the days immediately following a solar disturbance of the type which bombards the higher atmosphere with charged particles, as indicated by the international geomagnetic character figure. This response is found only during the winter season, during relatively inactive sunspot years (relative sunspot number <40), and reaches its peak on the second day after the particle invasion, gradually disappearing during the following five days. Craig[2] finds extensive statistical confirmation of Duell and Duell's results, indicated on the northern hemisphere by a reaction to higher pressure in the mean at 70° N, and to somewhat lower pressure in lower middle latitudes, in the days immediately following solar particle invasions. Duell and Duell, who worked only with the European-Atlantic sector, find the tendency to be one toward rising pressure in the Greenland-Iceland area, and towards falling pressure in an east-west zone in north-central Europe, indicating a tendency towards a southeastward shift of the primary Atlantic storm track.

Perhaps in some respects, the second part of Duell and Duell's study, which, to the best of this author's knowledge, has received no independent study or confirmation, is even more significant than the first part. The results were obtained from only a relatively short period of years for which data were available, but they are found to hold independently and almost equally for high and for low sunspot years, and for the summer and for the winter seasons. These results purport to show, during the days immediately following a solar ultraviolet invasion, as evidenced by intense chromospheric eruptions, a pronounced tendency at the 500 mb. level towards the buildup of a high pressure ride aloft centered over the Baltic-Finland area, and marked trough development west to southwest of the British Isles. This trend of the upper level pressure pattern represents an abrupt impulse towards a blocking pattern of the circulation of the hydraulic jump type discussed by Rex,[3] which he has shown to be characterized by predominantly warm dry conditions in Europe unfavorable to glaciation.

It should be noted in this connection that the marked warming of the climate and the glacial recession of the past 30 years has been characterized by increasing solar activity, culminating in record high sunspot numbers, and by a marked trend of the general circulation pattern over northern Europe and the north Atlantic towards this same blocking type (*cf.* Willett[4]). Any direct evidence linking solar ultraviolet activity to a blocking type reaction of the general circulation must be considered as highly significant.

One bit of statistical-synoptic evidence appears to speak against the point of view that maximum solar activity (as indicated by sunspot numbers) favors a nonglacial circulation pattern in Europe. In studying the changes of the general circulation of the northern hemisphere as related to the double sunspot cycle during the past century, Willett[5] finds that the change of the mean winter departures of pressure, temperature, and rainfall over northern Europe in passing to a major sunspot maximum from the preceding sunspot minimum definitely suggest a trend towards glacially favorable conditions. On the other hand, the change from sunspot minimum to a following minor maximum suggests a northward shift of a uniformly zonal wind pattern, a change which, according to Rex's study,[3] indicates the sunspot minimum pattern as being most favorable to glaciation. A study by Thorarinsson[6] of the year-to-year behavior of the principal glaciers of central Norway, which he claims is very closely paralleled also by the Alpine glaciers, shows that, during the 1900–1940 period of general glacial recession, the two periods of principal glacial recession were centered exactly on the two major sunspot maxima, while

the two periods of glacial readvance followed closely on the two sunspot minima which preceded minor sunspot maxima. This evidence points strongly to glacial recession at major sunspot maximum, and to glacial advance at times of relative sunspot inactivity. Hence, the conclusion reached in Willett's original study,[6] that major sunspot activity favors a glacial climate, is no longer accepted as valid, but rather the reverse relationship is indicated. Part of the explanation of the misleading indications of the initial study may lie in the fact that it was based only on winter circulation patterns, whereas the summer pattern probably is more decisive for glaciation. It appears that warm dry summers in the higher middle latitudes tend to predominate during periods of strong solar activity, whereas the winters are marked by contrasting extremes of warmth and coldness in the same latitude zone, a condition which is typically characteristic of the blocking pattern of the general circulation.

A further indication of the tendency for world weather patterns, particularly as they affect glaciation, to be influenced in their long term variations by the general level of solar activity as expressed by sunspots, is offered by a recent statistical study of Willett.[7] This study, covering the sunspot activity and the largest-scale features of the changes of the world weather patterns during the past two centuries, since 1750, suggests the following facts:

(1) Sunspot activity exhibits a pronounced 80-year cycle of alternating 40-year phases of maximum and of minimum activity, such that the mean sunspot numbers during the active periods are approximately double those during the inactive periods. The double sunspot cycle of alternating major and minor maxima appears to be a temporary phenomenon of a scant century's duration.

(2) The 40-year periods of high sunspot activity tend to be warm and dry in the lower middle, and warm and relatively wet in the higher middle latitudes, north of 50° N, corresponding to the past 30–40 year period of mildness. These are periods of poleward displacement of prevailing storm tracks, and of marked glacial recession in all latitudes.

(3) The 40-year periods of low sunspot activity tend to be cold and relatively dry in the higher middle latitudes, and cold and wet in the lower. These are periods of equatorward displacement of storm tracks and of increasing glaciation in most glacial areas. The transition from high to low sunspot activity takes place quite abruptly in the course of a single 11-year cycle, and the change in the world weather pattern is correspondingly sharp, whereas the transition from the inactive to the active solar phase, and the corresponding weather change, are relatively ill defined.

Final reference is made to a recent investigation by F. T. Bodurtha[8] which deals with the zone of polar anticyclogenesis in northwestern North America that corresponds to the Baltic-Finnish area of northwestern Eurasia. Several past statistical studies (Clayton, Schell, *etc.*) have indicated a positive correlation in this area between solar activity and pressure departures from normal.

For the purposes of this discussion, three phases of Bodurtha's study require mention, namely:

(1) The initial study of the incidence of polar anticyclogenesis as defined (an increase of the central pressure of an anticyclone $\geq$ 7 mb./24 hr.) shows the maximum frequency of occurrence for the entire northern hemisphere to be located in the upper Yukon valley, in the area selected for this study.

(2) The forty winters from 1899–1938 inclusive, covered by the Weather Bureau series of historical northern hemisphere weather maps, were grouped in three equal classes (13-14-13) of highest, intermediate, and lowest mean sunspot number (SS $>$ 50, SS 15-50, and SS $<$ 15). Northern hemisphere maps of mean departure from normal of sea level pressure were computed for these three groups of winter seasons. The greatest deviations of pressure from normal shown by these mean maps at any point on the northern hemisphere occur

exactly in this area of maximum anticyclogenesis in the upper Yukon, varying from +2.8 mb. for group 1 (SS > 50), to −0.3 mb. for group 2, and −2.2 mb. for group 3 (SS < 15). The statistical significance of these 13-year pressure departures in this particular area is further attested by the fact that the average absolute departure (disregarding sign) of winter pressure in this area is only 3.0 mb., whereas there are centers in several other regions of the northern hemisphere, including both the Aleutian and the Icelandic lows, where this mean annual deviation of the winter pressure exceeds 4.5 mb. Much smaller departures of pressure are found in these more active regions for the sunspot grouping of winter seasons than are found in the anticyclogenetic region under study.

(3) The frequency of anticyclogenesis in the area selected for study was checked against sunspot activity by the simple expedient of counting the number of cases of anticyclogenesis as defined, for each of the three sunspot groups of winters. The number of occurrences counted within the selected area is 67 for the high sunspot years, 53 for the intermediate years, and 31 for the years of lowest sunspots. The probability of obtaining such a distribution by chance is extremely small.

The following two highly tentative general conclusions are drawn from the studies referred to above, namely:

(1) Strong solar activity, as evidenced by high sunspots, favors in the mean poleward displacement of the zonal westerlies and of the prevailing storm tracks, as well as a blocking pattern of the general circulation, which is unfavorable to glaciation. Solar ultraviolet invasions of the atmosphere tend to induce an immediate reaction of the general circulation towards this high-latitude blocking pattern.

(2) Weak solar activity, as evidenced by low sunspots, favors in the mean an equatorward displacement of the zonal westerlies and of the prevailing storm tracks, as well as a zonal pattern of the general circulation, which is favorable to glaciation. Solar particle invasions of the atmosphere at time of low sunspots and at least during the winter season tend to induce an immediate reaction of the general circulation towards this low-latitude zonal pattern.

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ATMOSPHERIC PRESSURE CHANGES AND SOLAR ACTIVITY*

By RICHARD A. CRAIG†

This paper summarizes briefly a statistical study conducted by the author at Harvard College Observatory with the aid and advice of Dr. David Hawkins of the University of Colorado. The detailed results of the study will be described soon in a Special Report to the contractor. The purpose of this preliminary informal note is to bring the work, without delay, to the attention of interested persons and to stimulate discussion and criticism of the methods and results.

* The research reported in this paper was carried out by the author while he was at the Harvard College Observatory. The research was sponsored by the Geophysical Research Directorate of the Air Force Cambridge Research Laboratories under Contract W19-122-ac-17.

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Method of Analysis. The method of analysis follows that of Duell and Duell.* We selected the five days with highest geomagnetic character figure from each winter month during several years of low solar activity. Four winter months from each of eight years are considered, so that there are 160 such key days. For a given geographical location, the surface pressures on these 160 days are averaged. Similarly, an average pressure is obtained for the 160 days directly following the key days, one for the 160 days followed by two days the key days, and so on up to the average pressure for those days that follow by ten days the key days. The process is repeated for a large network of stations, covering the northern hemisphere between 30 and 70°N. The result is a large number of graphs that give the average variation of surface pressure following magnetically disturbed days at a number of points on the earth's surface.

As a second step, 160 different key days are chosen, these key days being the five days with the lowest geomagnetic character figure in each of the same months from which the first selections were made. The above averaging process is repeated, yielding the average variation of surface pressure following magnetically quiet days at the same points on the earth's surface.

The degree of magnetic disturbance reflects the state of solar activity. Magnetic disturbance of the type reflected in the magnetic character figure is generally believed to result from particle emission from the sun. It has also been correlated with the intensity of the coronal lines.† Quite conceivably, we may want to use a solar parameter to establish the key days in later phases of the study.

Results. The average pressure variation following magnetically disturbed days is compared with the average pressure variation following magnetically quiet days at each of the several geographical locations. A very striking negative correlation between the pairs of curves becomes apparent immediately. Where the pressure rises in the disturbed-day curve, it falls in the quiet-day curve, and *vice-versa.*

To measure this correlation, we have chosen a simple statistic which we may call "x." If, for a given location and a given number of days after the key day, the disturbed-day curve is above average for the location and the quiet-day curve is below average, or *vice-versa*, we assign x the value 1. If both are above average or both below average, x takes the value 0. We then average x over all geographical locations to obtain X.

For the key day and the day immediately following, we find X = 0.57. But X gets larger on succeeding days, finally attaining a maximum value of 0.75 on the fifth day. The value of X then becomes smaller and is 0.62 on the ninth day and 0.53 on the tenth day. For the second through the eighth days, X has an average value of 0.71. This value is the average of about 2000 separate values of x.

Statistical Significance of Results. To assess the statistical significance of these results we ask two questions:

(1) By our method of selection of data, have we introduced correlation between the pairs of curves?

(2) If there were no connection between geomagnetic activity and surface pressure, would we be likely to obtain the above results in a given random sample of this size?

Without going into detail here, we can say that the answer to the first question is "yes." Because the five magnetically quiet days in a month are necessarily different from the five magnetically disturbed days, the method of selection of data does introduce some correlation into the pairs of curves. Detailed consideration of this effect shows that the correlation so introduced could hardly cause X to exceed 0.60. The values of X on the key day and the first, ninth, and tenth following days (which average about 0.57) presumably show this effect, free from any geomagnetic effects.

On the second through eighth following days, however, X averages 0.71. We

* Smithsonian Miscellaneous Collections, Vol. 110, No. 8, 1948.

† ROBERTS & SHAPLEY. Ap. J. **103**: 257, 1946.

should like to know how many standard deviations separate this figure from the expected value of about 0.60. The computation of the standard deviation raises another complicating question. How many of the 2000 pairs that give the value 0.71 are actually statistically independent? Of course, the pressures from one day to the next and from one geographical location to a nearby location are interrelated.

A computation of the standard deviation, which takes account of these interrelationships, gives a value of about 0.017. This standard deviation would correspond to about 600 independent pairs, so that the interrelationships, or serial correlations, among the data reduce the effective number of pairs by a factor somewhat greater than three. The obtained value of 0.71, however, is still more than six standard deviations above the expected value of 0.60.

On a normal curve, a result six standard deviations away from the mean would be extremely unlikely. It seems safe to say that the average pressures obtained in this study have been influenced by their proximity to unusual geomagnetic conditions. Only some systematic selection factor that has escaped our notice could reasonably invalidate this statement. We have failed to find any beyond the one already taken into account.

Discussion of Results. The results discussed here are novel, to say the least. To our knowledge, this is the first time that a meteorological parameter in the troposphere has been related to conditions in the ionosphere in a reasonably definite fashion. With due respect to the Duells, who pioneered this work, their data were not numerous enough nor was their analysis complete enough to make their similar results statistically convincing.

We offer no physical explanation of the phenomenon at the present time. The results seem to say that surface pressures tend to develop into one pattern after a solar particle invasion, into an opposite pattern after quiet solar conditions. Perhaps the unusual geomagnetic conditions accompany other solar or atmospheric phenomena that actually affect the surface pressure. Perhaps the atmosphere may respond to the stimuli only under certain initial conditions. Perhaps, indeed, disturbed conditions may produce different pressure patterns at different times, so long as opposite pressure patterns follow corresponding quiet conditions.

We invite, indeed hope for, extensive criticism of the work, including possibly both adverse criticism of the statistical methods and sustaining criticism in the way of physical explanations of the results. Before this work can be considered satisfactorily complete, either the former must have invalidated the results or the latter must have put them on a sound physical basis. In the meantime, we propose to attempt both types of criticism and to extend the study statistically into other periods of time and with other solar and atmospheric parameters. The last-named project will be aimed at studying a much smaller number of cases, with a view to gaining an insight into the physical mechanisms involved.

POSSIBLE EFFECTS OF OZONOSPHERE HEATING ON SEA-LEVEL PRESSURE

By H. WEXLER*

This paper will be confined to theoretical results and observational verification of a particular mechanism originally suggested by B. Haurwitz[1, 2] as a possible connecting link between unusual solar activity and anomalies of the north-south sea-level pressure profile.

Departing from earlier assumptions of Clayton and others that the small observed (0.5 per cent) variations of the "solar constant" control large-scale pressure and weather anomalies, Haurwitz looked for an

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explanation through variations in intensity of those ultraviolet portions of the solar spectrum that are absorbed by ozone and thereby heat the ozone layer (roughly 20 to 50 km.). These sudden bursts of ultraviolet energy are supposed to occur more frequently (as much as 20 times) during sunspot maxima as compared to minima. Haurwitz found that the deployment of air away from the subsolar point (equator, say) in the suddenly heated ozonosphere and its consequent convergence poleward would give pressure rises as much as 20 mb in the zone from 20° to 30° latitude. There are several rather severe weaknesses in Haurwitz' calculation, however: (1) the rate of heating of the ozonosphere was taken so large that it amounted to an energy input equal to 10 solar constants; (2) the thermally produced pressure gradient at 20 km. (base of the ozonosphere) was kept constant during the readjustment process; and, most important, (3) transfer of *all* air above 20 km. from equatorial latitudes poleward was implicitly assumed.

In a paper by the writer, these three objections are eliminated.[3] Two models are considered in north-south profile: (A) a one-layer atmosphere, the central portion of which is heated suddenly by 10°C. throughout the whole depth, and (B) a double-layer atmosphere, the upper layer corresponding to the ozonosphere and accounting for five per cent of the total mass, and the lower denser layer (95 per cent of the mass) corresponding to the lower atmosphere not affected by absorption of ultraviolet energy. In Model B, a density discontinuity corresponding to a 9°C. inversion is taken originally between the two layers. The central portion of the upper layer is then heated suddenly by 10°C. Initially, there is no pressure gradient at the surface for both models.

In their final equilibrium phases, models A and B both show shifts of mass such that surface pressure falls at the subsolar point and rises poleward. In Model A, when the width of the central heated layer is taken as 36° of latitude, the maximum surface pressure drop is at the subsolar point, 9 mb, and as one proceeds poleward this changes to a much smaller and more uniform rise (<1 mb). At first glance, these pressure changes, particularly those near the subsolar point, appear impressive, but there are certain objections to Model A.

First, if the 10°C. heating of the central core is supposed to occur in 1 hour, then the

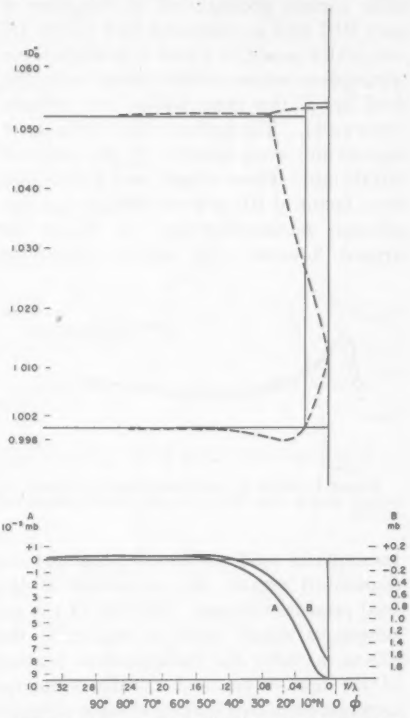

FIGURE 1. Upper figure, initial (full lines) and final (dashed lines) shapes of internal and free boundaries for Model B. Lower figure, change of surface pressure after adjustment from initial to final stages. Curve A refers to Model B; Curve B refers to Model A. Figure is symmetric about line y = 0.

energy-income rate required is equivalent to 3 solar constants, an unreasonable value. Second, the heating should actually be confined to the top five per cent of the atmosphere rather than spread throughout the entire depth. This will cut down the initial energy requirements to a reasonable six per cent of the solar constant (representing a doubling of the normal ultraviolet energy output rate of the sun). The surface pres-

sure drop at the subsolar point will now be much less than in Model A. This follows from the compensating effect of the upwelling of denser lower fluid into space originally occupied by the lower portion of the central, upper fluid, as this latter fluid diverges laterally under influence of the initial thermally-created pressure gradient. The subsolar surface pressure fall in this case is only 0.01 mb, as compared to a fall of 1.7 mb, which would be found in a single layer atmosphere whose central heated core was initially of the same width, 18° latitude (FIGURE 1). The pressure rises in the outer regions are even smaller, of the order of 0.0004 mb. These values, even if increased by a factor of 10, or even 100, are not significant meteorologically. It might be argued, however, that various simplifying

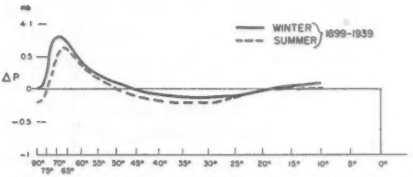

FIGURE 2. Profiles of the mean difference of pressure at sunspot maxima from that at sunspot minima, winter and summer.

assumptions made in the calculations have diminished greatly the magnitude of the final pressure-changes. But two of the assumptions should serve to maximize the effects, namely: the instantaneous heating of the ozone layer and the discontinuous termination of this heating at some distance poleward. Also, in the true atmosphere, the motions and pressure changes produced by the sudden ozonosphere heating must be superimposed on other motions and pressure changes present. These latter phenomena may completely mask the former effects, or, if this is not the case, they may tend to obliterate them.

It must be remembered, also, that these calculations are based on only one sudden impulse of solar energy capable of heating quickly a restricted portion of the ozonosphere by 10°C. If there are many repeated solar impulses, such as are believed to accompany periods of maximum sunspottedness, and, *if the results are additive*, these might cause a meteorologically significant shift of mass poleward, such that pressure is increased at higher latitudes and decreased at lower latitudes. Let us see if the observations of sea-level pressure, summarized according to sunspottedness, tend to support this reasoning.

In FIGURE 2 are shown the changes of the mean pressure profiles at sunspot maxima from those at preceding sunspot minima for

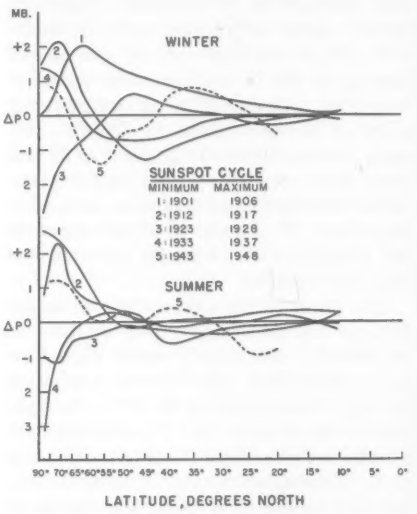

FIGURE 3. Profiles of the mean difference of pressure at sunspot maxima from that at sunspot minima, by individual sunspot cycles, 1899–1949.

winter and summer. The pressure data were taken from 5° or 10° intersections on daily northern hemisphere sea-level pressure charts for the period 1899–1939, comprising four sunspot cycles. Because of scarcity of synoptic reports in the tropics, the curves south of 25° or 20° latitude are uncertain. The summer and winter curves are quite similar and show falls at midlatitudes and larger rises at higher latitudes, maximum of $\frac{3}{4}$ mb at 70°N. Neglecting the uncertain positive pressure difference at low latitudes, the curves in FIGURE 2 agree qualitatively with the theoretical results. Con-

trary to the computations, however, the observed rise in higher latitudes is greater in magnitude than the falls in lower latitudes. This may be explained partly by the fact that the computations were carried out for an infinite plane earth. If a spherical earth had been used, the convergence of the meridians would have increased the computed rise at higher latitudes but not enough to be larger in magnitude than the falls at lower latitudes.

The close agreement between the winter and summer curves is particularly noteworthy.

To test the variability of the pressure differences by individual sunspot cycle, the average pressure-differences, sunspot maxima minus sunspot minima, were computed from the following sunspot cycles:* (1), 1905–1907 maxima (60) minus 1900–1902 minimum (6); (2), 1916–1918 maximum (80) minus 1911–1913 minimum (4); (3), 1927–1929 maximum (71) minus 1922–1924 minimum (12); (4), 1936–1938 maximum (104) minus 1932–1934 minimum (9); (5), 1947–1949 maximum (141) minus 1942–1944 minimum (19). The fifth cycle is not

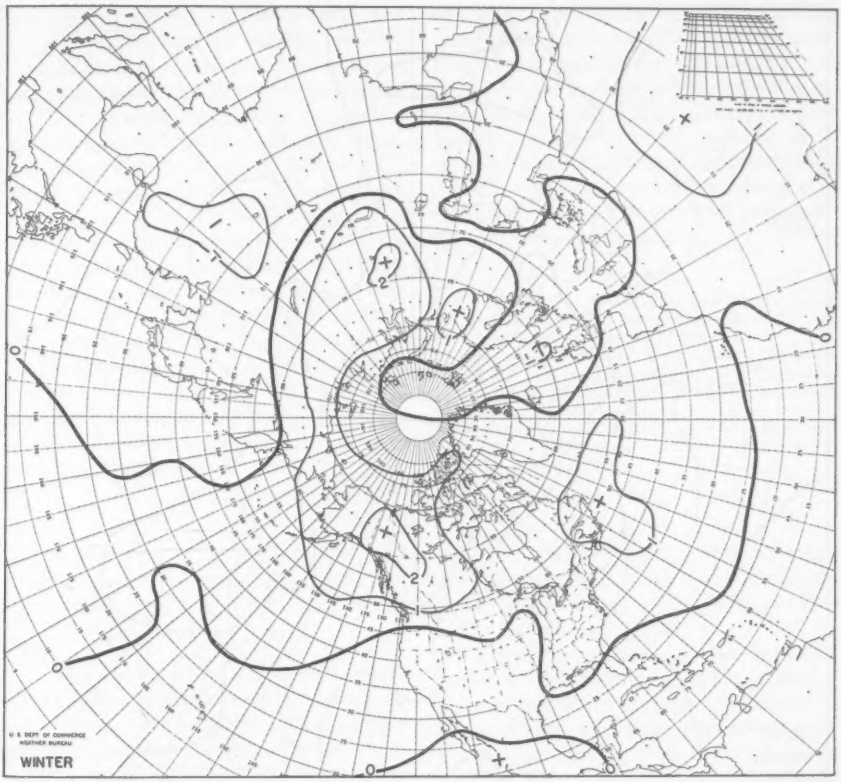

FIGURE 4a Mean difference of pressure (in mb.) at sunspot maxima from that at sunspot minima, Northern Hemisphere, 1899–1939, Winter.

* The maxima and minima sunspot periods were determined objectively by taking for each cycle those three consecutive years showing the greatest accumulated total of sunspot numbers and those three consecutive years having the least. The numbers in parentheses following the words "maximum" and "minimum" are the respective average annual sunspot numbers.

strictly comparable with the others, since the pressure data were taken only from the sector 0°W to 180°W. The results are shown in FIGURE 3, the upper portion of which is for winter and the lower portion for summer. For winter, the curves of sunpot cycles 2 and 4 are almost identical, cycle 5 seem to fall in a class by themselves. The winter difference-profiles for cycles 2 and 4 (characterized by "major" sunspot maxima), as distinguished from those for cycles 1 and 3 (characterized by "minor" sunspot maxima), agree with previous results of Hanzlik and Willett. However, the

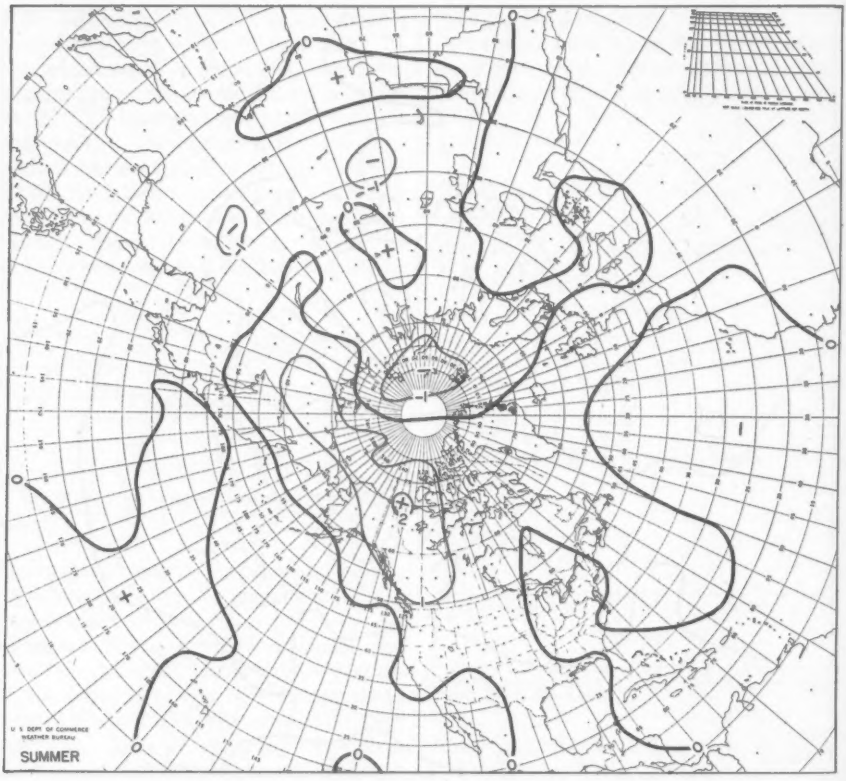

FIGURE 4b. Mean difference of pressure (in mb.) at sunspot maxima from that at sunspot minima, Northern Hemisphere, 1899–1939, Summer.

and show maximum values at high latitudes (70°N). They differ considerably from the curves for cycles 1 and 3, which show maximum values at middle latitudes (50° to 60°N) and minima at high latitudes. For summer, a different pair of cycles, namely numbers 1 and 2, are quite similar in that they show maxima at high latitudes, while cycles 3 and 4 show minima. The curves for change of similar difference-profiles of the sunspot cycles from winter to summer is puzzling.

In FIGURES 4a and 4b are shown the geographical distributions of the pressures at sunspot maxima minus those of sunspot minima for the entire period 1899–1939 during winter (October through March) and summer (April through September). In

winter, it is seen that the areas making the principal contributions to the rise at higher latitudes, as illustrated in the mean profiles in FIGURE 2, are northwestern North America and northeastern Siberia. These two regions are also favorite sites of winter polar anticyclones, and FIGURES 4a and 4b indicate that during sunspot maxima the average pressures in these regions may be as much as 2.5 mb higher than at sunspot minima. Most of Europe, the Norwegian Sea, and a portion of the Arctic Ocean is characterized by lower pressure during sunspot maxima than at sunspot minima, maximum difference amounting to 1 mb. At lower latitudes, the negative pressure differences are more uniformly distributed zonally. The pressure rise area in the interior of North Africa is uncertain because of lack of data. The summer pattern is quite similar to that of winter; in fact, the correlation between the two patterns is 0.53.

A further discussion of the sunspot-pressure relations, based on the historical northern-hemisphere map-series will be presented in the future by G. W. Brier, but the point to be made here is that, while there exists some evidence to indicate an excess of pressure at higher latitudes and a deficit at lower latitudes during sunspot maxima, it is not convincing, because of the lack of consistency from cycle to cycle and from winter to summer. The differences in the pressure profiles are small and of the same order of magnitude as differences obtained by selecting comparable groups of years at random, without any regard to sunspot numbers. Final solution of the problem will probably have to wait upon analyses of pressures taken during additional sunspot cycles in both the northern and southern hemispheres. The evidence thus far merely suggests, but does not prove, the existence of a weak solar (sunspot) control of large-scale sea-level pressure anomalies.

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SOME NOTES CONCERNING THE RELATIONSHIP BETWEEN SOLAR PHENOMENA AND WEATHER

By R. PENNDORF*

1. The energy which drives our atmospheric engine is received entirely from the sun. Thus, our "weather" is caused primarily by solar energy. Certain phenomena occur on the sun, sporadic in nature, which supposedly supply additional energy. Even if this energy may be small compared to the total output of energy, it may be just the kind to be absorbed in a distinct part of the atmosphere, where it is transformed into other forms of energy, and thus in turn may influence the atmospheric heat balance to appear finally in some unexpected "weather."

Most meteorologists will agree that such an effect is to be expected, but the majority of suggested relationships is still hypothetical in nature and unproven. I believe that such a relationship exists.

That solar phenomena are closely related to ionospheric events is well established. To say, however, that the "weather" is influenced by such solar phenomena, one either must prove that the ionospheric weather can influence the tropospheric weather by some physical process between the two spheres or find an absorption process which occurs in the troposphere or in the lower stratosphere. In the latter case, the disturbed heat balance in these layers will cause some anomalous tropospheric weather by an interacting process of some kind.

2. Let us turn to the second alternative first. Haurwitz proposed such a mechanism,[1] by assuming the additional solar

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energy to be absorbed within the ozone layer. If the spectral energy distribution during a flare can be represented by a black body radiation of a higher temperature than usual—as Haurwitz assumes, then the terrestrial atmosphere gets warmer and at the same time richer in ozone. Duetsch's figures show a 50 per cent increase of the amount of ozone if the solar radiation in the UV continuum increases from about 5000°K to 6000°K. Thus, one may check this hypothesis by investigating the measurements of the amount of ozone, whereas no measurements are available to check the heat balance between say 20 and 50 km.

What evidence exists for a connection between the amount of ozone (*i.e.*, the total amount above the surface) and solar phenomena? The only data available are measurements carried out with the Dobson spectral photometer at the ground. So let us use these to see what comes out. I must apologize for the few data I used, but very few observational results have been published during the last 20 years. The data I used at that time were kindly furnished by Dr. Goetz for Arosa, Dr. Toensberg for Tromsoe, and Dr. Werner for Aarhus in Denmark.

Let us start out with a simple statistical investigation between the amount of ozone and phenomena which are definitely solar in origin, namely the sunspot, solar flares, as revealed in SID and magnetic storms, and finally aurorae, which are caused by solar particle radiation.

It has been shown that the number of sunspots is correlated with upper atmospheric phenomena.[2]

2a. To begin with, I plotted the annual mean value of the sunspot number and the amount of ozone. Goetz did it in 1936,[3] and he suspected that an inverse correlation exists. At the same time, I doubted his statement very much, using all available basic data.[4] Early in 1943, when we approached a sunspot minimum, I investigated these two quantities again and came to the same conclusion, namely, that there is hardly any connection, but that there might be a very, very small inverse correlation.[5]

Goetz, having at his personal disposal more than 20 years of unpublished observations for Arosa discussed this matter in 1947,[6] showing the same result as I had, namely, the longer the series of observation the less convincing is any connection. He still holds the view, however, that there might be a very slight inverse correlation, but he has never published either the actual basic data or anything which substantiated this claim. That means *the total amount of ozone does not follow any solar cycle*, and we have to conclude that *the solar radiation does not change appreciably in the UV* between 1200–3000A, which produces ozone. I doubt very much that any complicated manipulation of the basic data will reveal more.

2b. The next studies were based on daily values, using the well-known superposed-epoch method. The day of the event is called 0 and the only question asked was whether the sign of the interdiurnal variation from the day -5 to -4, from -4 to -3 and so on through $+3$ to $+4$ was positive or negative. The amount of the change was not considered. The number of increases are added for each interval and expressed in per cent of the total number of cases. The aim was to find out whether this event leads to an increase or decrease of the amount of ozone.

The first element selected was a solar flare, which causes a sudden ionospheric disturbance (SID). A flare consists of solar wave radiation. The idea was that such a chromospheric eruption contains UV radiation which influences the ozone equilibrium. The results are given in TABLE 1.

There seems to be a tendency that, two days after the event, the ozone increases (at Tromsoe perhaps a day earlier). The statistics for 20 completely independent values puts the limits for random values between 27 and 73 per cent for a 95 per cent confidence limit and between 37–62 per cent for a 80 per cent confidence limit. Using the 80 per cent confidence limits would suggest

that results above 62 per cent may be considered as meaningful. Despite this statistical check, however, the number of cases is too small to be convincing.

The next element chosen was the geomagnetic storm. It is caused by solar particle radiation, not wave radiation. The definition chosen for a storm was a sudden commencement and an amplitude larger than 100 gammas, or in Potsdam range indices $K_1 \geqslant 5$ and $K_2 = s$. The result for Aarhus and Arosa is given in TABLE 2.

Thus, for middle Europe, the amount of ozone increased from the first to the second day after a magnetic storm occurred. It must be said again, however, that the number of cases available is very small, and I hesitate to draw far-reaching conclusions. The result agrees with the earlier given table for SID's, because a magnetic storm is normally preceded by a SID and, since the particles travel much slower than the wave radiation, they reach the earth about 24 hours later. For both groups of data, the predominant increase of ozone lags 2 to 3 days behind the day the event occurred at the sun, but the number of cases is far too small to consider the figures given in TABLE 1 and 2 as definite.

TABLE 1

INCREASE OF OZONE IN PER CENT OF THE CASES

	−5 to 0	*0 to +1*	*1 to 2*	*2 to 3*	*3 to 4*	*Statistical limits*	*Cases*
Aarhus/ Arosa	40	36	36	67	70	37–62	18
Tromsoe	49	56	*73*	41	50		12

An investigation between aurorae and the amount of ozone did not show any reliable result.

Karandikar[7] looked for a correlation of his Indian ozone measurements with magnetic character figures and found none. There was also no marked change in the amount of ozone when big sunspots and associated solar disturbances were reported in February and March, 1946.

3. Now comes the question: At what altitude should the ozone concentration be studied to prove whether any effect of the solar phenomena on the ozone concentration exists? Well, one has to go high up, about 40–50 km., because an effect will not be noticeable in the lower layers. This can be substantiated by the following considerations:

(a) Atmospheric ozone has some wonderful buffer mechanism which keeps its concentration more or less constant when the incoming radiation changes. The retarding processes are so strong that there is no measurable daily variation in the amount of ozone.

(b) The two calculations of the vertical ozone distribution carried out by Duetsch[8] for a 5000°K radiator, which comes very close to the real distribution of solar energy and for a 6000°K radiator, are used. The difference of the ozone concentration is given in TABLE 3, which demonstrates that the largest changes of ozone as compared to the total value are found between 22 and 37 km. In this interval, 92 per cent of the changes occur. If the changes in a particular layer are considered, however, then the increase is fairly constant between 35 and 80 km. and is very small below 30 km. The time of relaxation must be considered, too. It is very short, less than a day, above 40 km., but very long in lower layers; around 35 km., it takes about 6 days and at 30 km. about 140 days. This means that, around 30 km. and below, the equilibrium concentration is not controlled by solar phenomena but rather by weather; that is to say, that advection and vertical motions will bring about changes of the concentration very quickly compared to solar phenomena, but this is the region where the large contribution to the amount of ozone comes from.

TABLE 2

INCREASE IN THE AMOUNT OF OZONE IN PER CENT

Day					
−5 to −0	*0 to 1*	*1 to 2*	*2 to 3*	*3 to 4*	*Cases*
51	46	73	69	64	16

(c) The foregoing considerations indicate that it seems unlikely that solar phenomena can be detected in the observation of the amount of ozone. That is one reason why I have strong doubts about the statistical results given before. It is agreed, however, that one might detect some influence around 40 km. At this altitude, measurements are very difficult and are not carried out yet on a daily basis.

4. From the study of the relation between ozone and sunspot number, it was concluded that the solar UV radiation does not change appreciably. This statement can be made even stronger, because theoretical studies have shown that the radiation of a disturbed sun increases only in distinct lines but not in the continuum. For the region in question, only the Lyman α line intensity increases, but no increase is expected between 2000–3000Å to be absorbed by ozone below 40 km. The increase in the Lyman α-line would increase the amount of atomic oxygen between 80 to 100 km., but it would not penetrate much deeper. Thus, there is little hope right now for additional radiation coming from a disturbed sun to influence the heat balance of the stratosphere below 50 km., as Haurwitz has assumed.

5. If we acknowledge these conclusions, there remains only the first alternative, namely, an ionospheric influence on the troposphere. Static considerations seem to be unable to explain such a transfer of energy through the atmosphere. It seems possible only if one assumes a cellular structure in the atmosphere in which each cell influences the cell below by some kind of frictional forces. If the circulation in one cell increases, the circulation in the cell below increases too and *vice versa*. Thus, one might be able to connect the ionosphere and the troposphere, even if the efficiency of this mechanism is very small. Moreover, the general circulation undergoes annual changes in each layer. Thus, the atmosphere may favor or supress such transfer processes during specific seasons.

TABLE 3

Altitude in km.	*Difference in particle concentration/cm.*^{3}	*% of the total amount*		*% of the original concentration in this layer*	*Reduced time of relaxation*
85–75	0.0051 10^{13}	0.06		5.6	
75–65	0.0067	0.08		5.3	
65–55	0.0095	0.1		5.3	less than 1 day
55–47½	0.0097	0.1		4.9	
47½–42½	0.017	0.2		5.0	
42½–37½	0.089	1.0		5.5	
37½–32½	2.11	23.8		9.2	6 days
32½–27½	3.82	43.1	92%	4.0	32 days
27½–22½	2.18	24.6		1.8	140 days
22½–17½	0.50	5.6		1.1	
17½–12½	0.14	1.6		1.0	
12½–7½	−0.03	−0.3		0.9	

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MATHEMATICAL ASPECTS OF TORSIONAL BEHAVIOR IN CURVED BEAMS*

By JOHN G. HOTCHKISS†

"Mathematics," Bertrand Russell once said, "takes us into the region of absolute necessity, to which not only the actual world, but every possible world, must conform." In reality, those words by Russell express the true relation of mathematics to the more mundane sciences.

For many generations, the history of mathematical progress has been achieved, perhaps dominantly, by attacking the problems that are presented by science and nature to the applied mathematicians.

Numerous examples might be cited from scientific progress to describe the fruitful influence of theoretical physics upon pure mathematics. The essence of progress is sought in the interaction of mathematics and physics which, when continued, becomes deeply enriched.

Accordingly, we approach our engineering problem by exposing it to mathematical scrutiny. We shall not solve this problem. At best, we shall merely bare to one another the results of our exploratory hunting. Perhaps we shall find others who are in agreement with our assumptions or even accept qualifiedly the conclusions of our prolix study.

Passing over these mathematical influences, let us push forward and inquire into the mathematical aspects of torsional behavior in curved beams.

Our first realization is that the old memoir of curved beam theory developed in 1855 is still indisputable. This theory by the famous mathematician, St. Venant, was a classical memoir. Its dignity rests in the well known derivations for circular sections. With the passing of less than a decade, Föppl extended the theory to noncircular sections composed mostly of three rectangles approximating an I-profile.

The fundamental concepts of torsion have not changed in the last one hundred years in regard to rectangular and circular cross sections. The work of Timoshenko and of Beran and Unold has greatly extended the treatment to circular and curved beams of I-profile. It was not until 1936 that the experimental work of Lyse and Johnston led to the determination of the torsional constant K for American rolled structural sections.

The application of these concepts and derivations to problems of torsional behavior becomes highly complex. Practicing engineers confronted with the design of a circular ring girder are choked at the start with a tremendous nebula of time-consuming computations. Hence, it is necessary to set up this problem in a manner that is amenable to mathematical treatment, in a form that can be conveniently and quickly handled. As in any engineering problem, a primary requisite for a comprehensive solution is the analysis of the physical systems as idealized bodies described mathematically in terms of their dominant properties. The dominant properties are known by either experience or measurement.

If any physical problem were set up as precisely as possible in mathematical form, its solution would be too difficult. Herein lies the inestimable importance of making simplifying assumptions. The analysis of curved structural beams is simplified first by making certain assumptions. Let the material of these beams be considered as steel, then they are treated as elastic bodies. When they are deformed from an initial state A to a state B under the influence of forces, they resume their initial state A when these forces are removed. By "state," we refer to the geometric shape and physical properties, namely, density and stress.

We are concerned in this paper with beam

* This paper, illustrated with lantern slides, was presented at the meeting of the Section, April 20, 1951.

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profiles that are elastic bodies considered to be initially of the same material throughout and of constant density, that is, homogeneous and isotropic. The elastic properties are the same in all directions.

A frequent expression appearing in torsional problems is the shear modulus G. Experimentally, we know that Poisson's ratio for steel is 0.303, from which we can write the relation

$$G = \frac{E}{2(1 + 0.303)}$$

or

$$E = 2.6.G$$

An accepted assumption, justified experimentally, is that the moment of inertia of the cross section of one flange about the minor or y–y axis of an I-beam is one-half of the moment of inertia of the cross section of the entire beam about its y–y axis.

From these assumptions, it is possible to develop a logical physical and mathematical picture of the problem employing the basic laws of static equilibrium.

An illustration of the many variables in a curved beam is evidenced by the following example. Under the influence of an external load P applied at the center line of a beam curved about its weak axis in a horizontal plane, there exists: an internal moment M_i, resulting from the longitudinal stresses in the extreme fibers of the member; a resulting vertical shear force V_i; and a resulting bending moment M_b in each curved flange, with an associated lateral shear force V_b as well as a torsional moment M_t.

In order that the moments and shears satisfy the static equilibrium requirement, the following is true:

$$Pa - M_i = 0 \quad (1)$$
$$Pd - V_b = 0 \quad (2)$$
$$P - V_i = 0 \quad (3)$$

where a and d are the moment arms or Cartesian coordinates of the load P.

A small portion of this beam of length $dx = rd\phi$ is loaded with a uniform load w per unit length. Then, by taking a summation of moments about the x and y axes and differentiating, a series of simultaneous differential equations may be written which reduce to

$$y = C_1 + C_2\phi + C_3 \sinh \epsilon\phi + C_4 \cosh \epsilon\phi + C_5 \sin \phi + C_6 \cos \phi + C_7 \phi \sin \phi + C_8 \phi \cos \phi + \frac{wr^2}{2}\phi_2 \quad (4)$$

in which ϕ is the angle between the support and the point dx, r is the radius, and w the unit load.

The term ϵ may be expressed as

$$\epsilon = r \sqrt{\frac{2GK}{E\frac{I_y}{2}h^2}}$$

which can be simplified by letting $G/E = 0.385$. Then

$$\epsilon = \frac{1.24}{h} r \sqrt{\frac{K}{I_y}}. \quad (5)$$

The Problem. By what mathematical techniques can the complexity of torsional behavior be reduced to a logical method for quickly computing the primary and secondary effects?

Primary effects of torsion are considered as the bending stresses resulting from simple flexure. Expressed mathematically, $\sigma_1 = \frac{Mc}{I}$. In addition to this primary bending stress another important aspect, secondary bending, is introduced. The secondary stress σ_2 must be added algebraically to σ_1. The secondary stress may be simply expressed as $\sigma_2 = \frac{M_b}{\Omega}$ where $\Omega = \frac{S_y}{2}$.

Stresses on any particle of a beam are the result of the beam being subjected to bending and shearing action. It is the secondary bending developed in the flanges of circular ring-beams that is of principal concern to us. It has been demonstrated in actual structural problems that this secondary bending moment can produce bending stresses frequently in excess of the primary stresses.

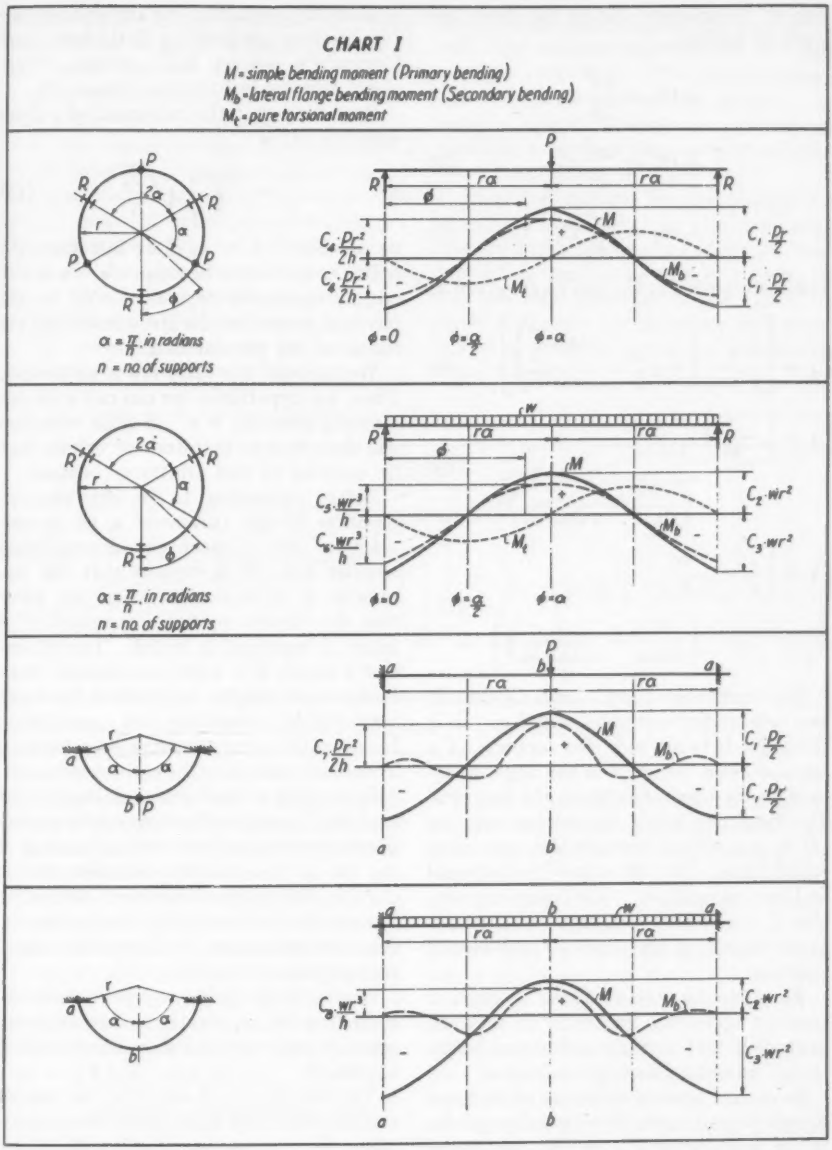
CHART I
M = simple bending moment (Primary bending)
M_b = lateral flange bending moment (Secondary bending)
M_t = pure torsional moment
$\alpha = \frac{\pi}{n}$ in radians
n = no. of supports
$\alpha = \frac{\pi}{n}$ in radians
n = no. of supports

We will introduce the expressions for lateral or secondary bending in a circular ring which is loaded with (1) a concentrated load P symmetrically applied between supports and (2) with a uniform load w per unit length. CHART I illustrates the relation-

ship of the moment curves for these two types of loading.

$$\pm M_b = \frac{Pr^2}{2h} C_4 \tag{6}$$

$$+M_b = \frac{wr^3}{h} C_5 \tag{7}$$

$$-M_b = \frac{wr^3}{h} C_6 \tag{8}$$

The coefficients C_4, C_5, and C_6 are given as

$$C_4 = \left[\frac{1}{1+\epsilon^2} \left(\tan \frac{\alpha}{2} - \frac{\tanh \frac{\epsilon\alpha}{2}}{\epsilon} \right) \right] \tag{9}$$

$$C_5 = \left[-\frac{1}{\epsilon^2} + \frac{\alpha}{1+\epsilon^2} \cdot \left(\frac{1}{\sin \alpha} + \frac{1}{\epsilon \sinh \epsilon\alpha} \right) \right] \tag{10}$$

$$C_6 = \left[\frac{1}{\epsilon^2} - \frac{\alpha}{1+\epsilon^2} \cdot \left(\frac{1}{\tan \alpha} + \frac{1}{\epsilon \tanh \epsilon\alpha} \right) \right] \tag{11}$$

The coefficients C_4, C_5, and C_6 contain two independent variables, ϵ and α. 2α is the angle between any two supports of a circular beam, hence, α is the angle measured to the bisector between the supports. The term previously defined by equation (5) is a torsional constant for any given rolled shape. It is the ratio of the torsional and flexural rigidities. The dependent variable C is not only a function of ϵ, but is also a function of $\sin \alpha$, $\tan \alpha$, $\epsilon \tanh \epsilon\alpha$, and $\epsilon \sinh \epsilon\alpha$.

The Approach to the Problem. A physical constant appearing frequently in torsional analysis of both straight and curved beams is the "secondary bending constant a."

Secondary bending resistance of the beam flanges depends upon the physical properties of the beam material. In the case of steel beams whose elastic and shear rigidities are known, the bending constant is expressed as

$$a = 0.806h \sqrt{\frac{I_y}{K}} \tag{12}$$

(Values of "constant a" for any given structural section are listed in Bethlehem Steel Company's booklet S-57 entitled, "Torsional Stresses in Structural Beams.")

By noting that the reciprocal of ϵ from equation (5) is

$$\frac{1}{\epsilon} = \frac{0.806h}{r} \sqrt{\frac{I_y}{K}} \tag{13}$$

we see that $1/\epsilon = a$, if we introduce the radius r, which then becomes $r/\epsilon = a$ or $\epsilon = r/a$, an expression dependent only on the physical properties of a given beam and the radius of the circular ring.

We can call the ratio r/a a parameter. Then, by hypothesis, we can call ϵ the parameter, since $r/a = \epsilon$. A little reflection will show that an infinitude of values may be assigned to this arbitrary constant.

Before proceeding to the objective application of the parameter ϵ, let us consider for the moment the dimensionless number r/a. It is evident that the parameter is dimensionless when we know that the dimension of the "constant a" is length L expressed in inches. The parameter ϵ equals L/L when we consider Newtonian mass, length, and time as the basic measureable quantities of mechanics. Inches in the parameter have been replaced by symbols indicating the entities measured.

In making a mathematical engineering analysis, it is often advantageous to employ dimensionless numbers. One advantage of the use of dimensionless numbers lies in changing the system of measure. We might measure the parameter ϵ by the centimeter-gram-second system; the numerical magnitude remains the same.

Coming back again, we repeat the statement that the parameter ϵ is an arbitrary constant which can be assigned an infinitude of values.

The importance of selecting the parameter ϵ is more fully appreciated when we attempt to analyze over 137 wide flange sections rolled today. For every selected beam, the following independent variables, appearing in the lateral bending equations, remain constant for a given beam selection: the height h between flange centroids and

the torsional bending constant "a." The independent variables of loading, w and P, the radius r of the circular ring, and the angle α are independent of and not a function of any given rolled section.

It was conceived that the parameter ϵ might be assigned limiting values of numerical magnitude that would encompass the entire range of rolled structural shapes. The maximum limit of the "torsional constant a" was found to be $1/a = 0.0328$ for an 8WF15 beam and the minimum $1/a = 0.00952$ for a 30WF124 beam.

The remaining term r in the parameter must arbitrarily be limited for any given rolled section. Shop fabrication methods employed in bending the curved beam and secondary bending stresses dictate the upper and lower limits of radius r.

Computations for the coefficient C in equations (9), (10), and (11) become necessarily tedious and require extreme accuracy. Customary slide rule results are inadequate, and the use of hyperbolic tables of six or seven places is imperative. As the product of the variables $\epsilon\alpha$ increases, sinh $\epsilon\alpha$ and cosh $\epsilon\alpha$ increase very rapidly. The value of $\frac{1}{\sinh\epsilon\alpha}$ converges very severely to zero.

The solution to the problem of determining the lateral bending moment M_b lies in a direct procedure for finding values of the coefficient C. The numerical magnitude of C may be quickly determined from curves similar to FIGURE 1. Three separate plots of equations (9), (10), and (11) were set up with the ordinate representing the independent variable α. The dependent variable C was plotted as the abscissa. By using a logarithmic scale for ordinate and abscissa, a straight line curve then represents the variable parameter ϵ.

From the equation for the coefficient C as plotted in FIGURE 1 for maximum negative M_b due to w load per unit length, some interesting conclusions may be drawn. In the treatment of stress intensity, it is desirable to reduce the secondary unit stress, since it is the major part of the total bending stress intensity.

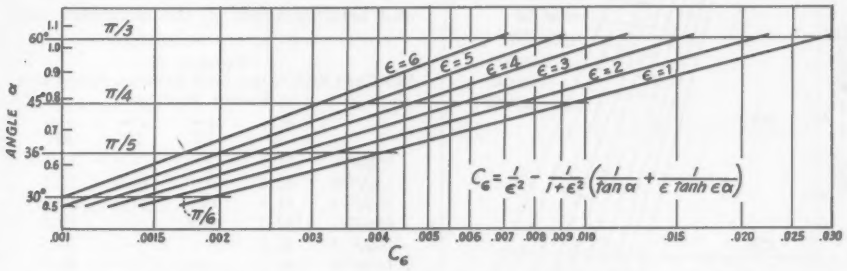

FIGURE 1. Lateral bending moment coefficient C_6 for uniform loading when maximum moment $-M_b = \frac{wr}{h} C_6$. Angle α is expressed in radians. When the number of supports equals n then $\alpha = \frac{\pi}{n}$ *viz.*, for three supports $\alpha = \frac{\pi}{3}$, or 60°. $\epsilon = r/a$ where r is the radius in inches and "a" a torsional constant is expressed in inches.

It becomes evident that, if the coefficient C were reduced, M_b, being directly proportional, might be reduced likewise, provided the variable h, a constant for any given section, didn't decrease at a greater rate than C increased. It must be remembered that C is dependent upon the parameter ϵ which contains the variable r, and "a," which is also a constant for any given section.

If we permit the coefficient C to decrease, then we may vary the variables ϵ and α. Apparently, from the curves, we see that we could reduce the angle α, or in other words we could increase the number of supports for the circular ring beam. It is likewise seen that an increase in parameter ϵ results in a decreased value for C. When ϵ is increased what happens to the ratio r/a? We know, for any given beam sec-

tion, that "a," a physical property must remain constant, hence r must increase. Conversely, when r is held constant, "a," being indirectly proportional, must decrease.

The lateral bending moment M_b is a function of w, r,[3] and $1/h$, as well as the coefficient C. The radius r likewise is a function of ϵ, which in turn is a function of C. The question then arises as to the rates at which the independent variables influence the magnitude of the lateral bending moment.

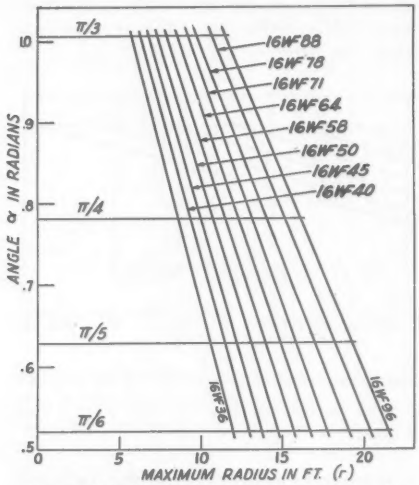

FIGURE 2. Maximum radius r in ft., which, for a given angle α, will not produce a combined unit bending stress in excess of 20,000 p. s. i. All beams to the right of any coordinate point are safe. The beam to the right and nearest this point is the most economical choice.

To answer this question, additional knowledge of the functional behavior of variable r was necessary. It is evident that the variable r influences the lateral bending moment as well as the flexural moment, each of which produces a bending stress. But, what maximum value of r will yield a combined unit bending stress not in excess of the allowable unit stress of 20,000 p. s. i.?

An analysis of several wide flange sections rolled by American mills provided information for the preparation of FIGURE 2. The curves of this figure represent all the wide flange beams in the 16-inch series. Angle α, in radians, was plotted as the ordinate against the maximum radius r in feet as the abcissa.

From FIGURE 2, one may determine the maximum radius for a selected beam when angle α, the angle from one support to the bisector between supports, is given. The curves were plotted for the case when $w = 1$ kip per ft. With an increase in load w, the entire family of curves moves laterally to the left, resulting in a decreased value for the maximum radius. Conversely, a decrease in w moves all curves to the right.

A study of FIGURE 2 leads to another question. What effect does angle α produce on the primary and secondary unit bending stresses? If we let σ_1 equal the primary unit bending stress, σ_2 the secondary unit bending stress, and σ_T the total combined bending stress, we can express in per cent the primary stress as σ_1/σ_T and the secondary stress ratio as σ_2/σ_T. Plotting σ_2/σ_T as the ordinate and σ_1/σ_T as the abcissa, we find that the 16-inch series of wide flange beams falls into a well-defined group for a given angle α. The results of such plotting are tabulated in TABLE 1.

TABLE 1
PER CENT SECONDARY UNIT BENDING STRESS DUE TO FLANGE BENDING

	$\alpha =$ 60°	$\alpha =$ 45°	$\alpha =$ 36°	$\alpha =$ 30°
16WF96	81.4	—	—	33.1
16WF88	81.4	65.0	48.0	33.1
16WF78	81.8	65.5	49.3	32.0
16WF71	82.7	65.5	—	36.5
16WF64	82.3	—	—	36.5
16WF58	82.7	—	—	37.0
16WF50	84.5	—	—	39.5
16WF45	84.2	—	—	39.5
16WF40	84.7	—	—	39.5
16WF36	85.5	—	—	40.1

It can be concluded from TABLE 1 that decreasing angle α likewise decreases the per cent of lateral bending stress σ_2. Decreasing the weight per linear foot of the beam increases the secondary bending stress. This conclusion might have been drawn previously, since we know that the flange material, offering resistance to lateral bending, is decreased proportionately with a reduction in weight per linear foot.

Summary. Through mathematical analysis, the complex torsional behavior of curved beams and the associated computations have been simplified. Much of the cut and try method of selecting beams bent about their weak axis to resist lateral and flexural bending is alleviated. It is now possible to select economically a curved beam to support a given loading condition with the radius of curvature and the number of supports also given. Only in rare instances of heavy concentrated loading is it necessary to investigate shear stresses. The controlling criterion of stress analysis in curved beams is the direct bending stresses in the flanges.*

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* A detailed and documented treatment of the subject matter discussed in this paper covering the entire range of rolled beam shapes will appear in a succeeding publication by the same author.

THE NEW PHYSICAL ANTHROPOLOGY*

By S. L. WASHBURN†

Recently, evolutionary studies have been revitalized and revolutionized by an infusion of genetics into paleontology and systematics. The change is fundamentally one of point of view, which is made possible by an understanding of the way the genetic constitution of populations changes. The new systematics is concerned primarily with process and with the mechanism of evolutionary change, whereas the older point of view was chiefly concerned with sorting the results of evolution. Physical anthropology is now undergoing the same sort of change. Population genetics presents the anthropologist with a clearly formulated, experimentally verified, conceptual scheme. The application of this theory to the primates is the immediate task of physical anthropology.

In the past, physical anthropology has been considered primarily as a technique. Training consisted in learning to take carefully defined measurements and in computing indices and statistics. The methods of observation, measurement, and comparison were essentially the same, whether the object of the study was the description of evolution, races, growth, criminals, constitutional types, or army personnel. Measurements were adjusted for various purposes, but measurement of the outside of the body, classification, and correlation, remained the anthropologist's primary tools. The techniques of physical anthropology were applied to a limited group of problems and any definition or statement of traditional anthropology must include both the metrical methods and the problems for which the methods were used. Further, anthropology was characterized by theories, or rather by a group of attitudes and assumptions.

There has been almost no development of theory in physical anthropology itself, but the dominant attitude may be described as static, with emphasis on classification based on types. Any such characterization is oversimplified, and is intended only to give an indication of the dominant techniques, interests, and attitudes of the physical anthropologist. Except for emphasis on particular animals, physical anthropology shared much with the zoology of the times when it developed. Much of the method was developed before the acceptance of the idea of evolution, and all of it before the science of genetics.

Physical anthropology should change, just as systematic zoology has changed. The difficulties which accompany the necessary modifications can be greatly reduced if their nature is clearly understood. Naturally, in a time of rapid flux there will be numerous doubts and disagreements as to what should be done. This is natural, and what I have to offer is a tentative outline to indicate how parts of the new physical anthropology may differ from the old.

The old physical anthropology was primarily a technique. The common core of the science was measurement of external form with calipers. The new physical anthropology is primarily an area of interest, the desire to understand the process of primate evolution and human variation by the most efficient techniques available.

The process of evolution, as understood by the geneticist, is the same for all mammals. The genetic composition of a population may be described in terms of gene frequencies. The modification of these frequencies results in evolution which is caused by *selection*, mutations, drift, and migrations. Mutations and migrations introduce new genetic elements into the

* This paper, illustrated with lantern slides, was presented at the meeting of the Section, April 23, 1951.

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population. But selection on the phenotype, adapting animals to their environment, is the primary cause of alteration in gene frequencies.

This is essentially a return to Darwinism, but with this important difference: Darwin wrote in a pregenetic era. Therefore, he did not understand the mechanism which makes possible the production of variation and the possibility of selection. Since Darwin's ideas could not be proved in detail by the techniques available in his time, the concept of selection did not become fully effective. Therefore, some pre-evolutionary ideas continued in full force. More Linnaean species were described from types after Darwin than before. The idea of evolution created interest in species, but the species were described in pre-evolutionary terms. Further, it is possible for people to hold a variety of theories in place of, or in addition to, Darwin's. For example, Lamarckian ideas have continued right down to today. Orthogenesis has been widely believed and irreversibility has been regarded as a law.

It has been claimed that evolution should be described in terms of nonadaptive traits, yet this is impossible if evolution is largely due to selection. The first great achievement of the synthesis of genetics, paleontology, and systematics is in clearing away a mass of antiquated theories and attitudes which permeate the writings of the older students of evolution. Further, the new evolutionary theory shows which aspects of past work are worth using, extending, and strengthening. This is possible because much of the mechanism of evolutionary change is now understood, clearly formulated, and *experimentally verified*. The logic of Darwin's great theory could only become fully effective when techniques had been developed to prove that selection was right and that other ideas of evolution were wrong. A change in theory, no matter how popular, is not enough. The new ideas must be implemented by effective techniques.

If a new physical anthropology is to differ effectively from the old, it must be more than the adoption of a little genetic terminology. It must change its ways of doing things to conform with the implications of modern evolutionary theory. For example, races must be based on the study of populations. There is no way to justify the division of a breeding population into a series of racial types. It is not enough to state that races should be based on genetic traits; races which can not be reconciled with genetics should be removed from consideration. If we consider the causes of changes in gene frequency as outlined above, and if we are concerned with the process of evolution, the task of the anthropologist becomes clear. He has nothing to offer on mutation, but can make contributions with regard to migration, drift, and selection.

The migrations of man made possible by culture have vastly confused the genetic picture. Before selection can be investigated, it is necessary to know how long a people has been in an area and under what conditions they have been living. For example, the spread of European people, of Bantu speakers, or of Eskimo, all have changed the distribution of the blood groups. The interpretation of the genetic situation demands an understanding of history. Whether people became adapted to cold by selection or by change in their way of life completely alters the interpretation of the distribution of physical traits. This has been widely recognized by anthropologists, and the solution of this difficulty requires the active collaboration of archeologists, ethnologists, linguists, and students of the physical man.

Drift is related to population size, and this depends on the way of life. Again, as in the case of migration, the situation in which drift may have taken place cannot be specified by the physical anthropologist alone, but requires the active collaboration of many specialists. The adoption of modern evolutionary theory will force a far closer and more realistic collaboration between the branches of anthropology than ever before.

Although much of the present distribution of races may be explained by migration and although drift probably accounts for some differences, selection must be the explanation of long term evolutionary trends and of many patterned variations as well. Anthropologists have always stressed the importance of adaptation in accounting for the differences between apes and men, and sometimes have used the idea in interpreting racial divergences. But suggestions of adaptations are not enough. It is easy to guess that a form is adaptive, but the real problem is to determine the precise nature of a particular adaptation. The work I have been interested in is designed to demonstrate the relation of form to function. My feeling has been that it is impossible to do more than guess about this matter using traditional anthropological measurements, and that the literature is already too full of uncontrolled speculations. Therefore, I would like to take this opportunity to present an outline, a beginning, of an analysis of the human body into complexes which may vary independently.

In this work, the guiding principle has been that the major force in evolution is selection of functional complexes. A variety of methods has been used to demonstrate the adaptive complexes. The four major methods for factoring complexes out of the body are: (1) comparison and evolution; (2) development; (3) variability; and (4) experiment. All these have been used by numerous investigators, but, to the best of my knowledge, they have not been combined into a working system. All must be used to gain an understanding of the human body.

FIGURE 1 shows the body divided into the major regions, which seem to have had remarkable independence in recent evolutionary history. The complex to attain its present pattern first is that of the arms and thorax. This complex is associated with arm swinging in the trees, the way of life called "brachiation." It is association with a reduction in the deep back muscles and in the number of lumbar vertebrae and consequent shortening of the trunk and elongation of all parts of the upper extremity, adaptation of the joints and muscles to greater pronation, supination in the forearm, and flexion and abduction at the shoulder. Many changes in the positions of viscera are associated with the shorter trunk. We share this complex with the living gibbons and apes. The bipedal complex was the next to develop and seems to have been fundamentally human in the South African man-apes. The major changes are in the ilium and in the gluteal muscles. Just as in the arm, the change is in a bone-muscle complex, which makes a different way of life possible. The head seems to have attained essentially its present form during the fourth glacial advance, perhaps 50,000 years ago. The brain continued to enlarge until the end of the last interglacial period, and the face decreased in size for some time after that. The great increase in the size of the brain and decrease in the face was after the use of tools.

Evolution, in a sense, has dissected the body for us, and has shown that great changes may occur in arms and trunk, pelvis and legs, and brain case, or face, accompanied by little change in the rest of the body. The first two complexes to change are related to brachiation and bipedal locomotion. The final changes in the head may well be related to changed selection after the use of tools.

To carry the analysis further, it is necessary to deal with one of the areas suggested by this preliminary dividing of the body. Let us consider the face, and especially the lower jaw. FIGURE 2 shows a lower jaw divided into regions which can be shown to vary independently by all the methods of analysis suggested before. The coronoid process varies with the temporal muscle. The angle of the jaw varies with the masseter and internal pterygoid muscle. The tooth-supporting area varies with the teeth. The main core of the jaw is affected by hormones which do not affect the other parts, as shown in acromegaly. Alizarin dye, which stains the growing bone, reveals

FIGURE 1.

the pattern of growth. The split-line technique (Benninghoff) shows the mechanical arrangement.

After making an analysis of this kind, comparisons of a different sort are possible. The simple statement, that a trait is or is

not there, is replaced by the attempt to understand under what conditions it might be present. For example, if the simian shelf is developed in monkeys and apes when the jaws are long and the anterior teeth large, then the South African manapes and other fossil men would not be expected to have such a shelf. The dental characters necessary to bring out the expression of the shelf are absent in all, except the Piltdown jaw. It can be argued that we have the potential for a simian shelf but that we do not have the necessary tooth and jaw size to make it evident. Trying to understand the process which produces a trait leads to very different

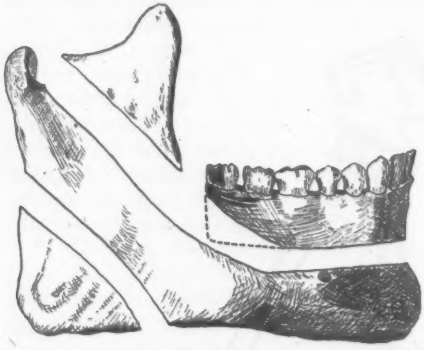

FIGURE 2.

evaluations than does a listing of presence or absence.

In the light of this sort of information, let us look at the skull of an Eocene lemur, *Notharctus*. The jaw is long, in conformity with the length of the teeth. It is low, and there is a large angular region. This region has been described as lemuroid. If this angle has remained there for 50 million years, however, over countless generations of lemurs, it must have more of a function than to mark the jaw as primitive or to help us in identifying lemur jaws. If the mandible of a remarkably similar modern lemur (genus *Lemur*) is examined, it is found that the internal pterygoid muscle inserts at the end of the angle, but that the masseter muscle inserts only on the lateral side of the ascending ramus, leaving the angle bare of muscle. An internal pterygoid muscle inserting in this position is a protruder of the jaw. The function of the angle of the lemur jaw is to provide insertion for a large, functionally important muscle. The dependence of the angular process on the internal pterygoid and the exact function of the internal pterygoid need to be experimentally verified.

The only point to be stressed now is that the theory that such a process is of adaptive significance, and that it is maintained by selection, leads one to look for a functional complex. If such a process is regarded simply as a taxonomic aid, or as nonfunctional, no guide is available for research or future understanding.

The post-orbital bar of this same lemur again illustrates the advantage of assuming, until it is proved otherwise, that a part is functionally important. Originally, the complete bony ring around the orbit may have been for protection or for some other unknown function. Once the ring is established, however, the skeletal framework for radical modification of the skull is present. The change from the lemur skull, with a wide interorbital region, to the monkey skull, with reduced olfactory mechanism and reduced interorbital space, is mechanically possible because pressure, tension, and buttressing of the sides of the face is provided by the complete rings of bone around the orbits. Structures which probably developed as part of a protective mechanism were pre-adaptive for a reorganization of the face.

Classic Neandertal man differs from other fossil men in that the angle of the lower jaw is poorly developed, the part of the malar bone associated with the origin of the largest part of the masseter muscle is small, and the lateral part of the brow-ridge is less sharply demarcated. All these differences may be related, and certainly the association of the small angle and malar suggest that the masseter muscle was small compared to the temporal muscle. Differences of this sort should be described in

terms of the variation in the groups being compared. Since similar difference may be found in living men, the development of appropriate quantitative, descriptive methods is merely a matter of time and technique. The procedure is: (1) diagnose the complex; (2) develop methods appropriate to describe variations in it; and (3) try to discover the genetic background of these variations.

So far, we are still engaged in finding the complexes, but even at this level it is possible to make suggestions about fossil men. Probably some Mongoloid groups will have the highest frequency of the big masseter complex, and some of the Negro groups the lowest. This is merely stating some traditional physical anthropology in a somewhat different way by relating statements about the face to those on the lower jaw and relating both to a large and important muscle. It differs from the traditional in the technique of analysis and avoids speculation of the sort which says that the characteristics of the Mongoloid face are due to adaptation to cold.

In this preliminary analysis of the lower jaw, the attempt has been made to divide a single bone into relatively independent systems and to show that the differences make sense in terms of differing adaptations. Eventually, it may be possible to understand the genetic mechanisms involved. If this type of analysis is at all correct, it is theoretically impossible to make any progress in genetic understanding by taking the traditional measurements on the mandible. They are all complex resultants of the interrelation of two or more of the variables. The measurements average the anatomy in such a way that it is as futile to look for the mode of inheritance of the length of the jaw as it is to look for the genes of the cephalic index.

The implications for anthropology of this type of analysis may be made clearer by some comparisons of the skulls of monkeys. If the skulls of adult male and adult female vervets are compared, many differences may be seen. The male skull is larger in all dimensions, particularly those of the face. If, however, an adult female is compared to a juvenile male with the same cranial capacity and the same weight of temporal muscle, all the differences disappear, except that in the size of the canine tooth. What would appear to be a very large number of unrelated differences, if traditional methods were used, are only aspects of one fundamental difference in the size of the face. If a large-faced monkey is compared with a small-faced one, both of the genus *Cercopithecus*, there appear to be many differences. Yet again, if animals of the same cranial capacity and the same temporal muscle size are compared, almost all the measurements are the same. The species difference is in quantity of face, although this appears in many different forms. If these two skulls were fossil men, differing in the same way, and if they were treated by the usual anthropological methods, they would be found to differ in numerous observations, measurements, and indices. Yet one may be transformed into the other by a simple reduction in mass of face (including teeth, bones, muscles). Perhaps many fossils are far less different than we have supposed. The methods used created the number of differences, just as a metrical treatment of these monkeys would make the adults appear very distinct.

The purpose of this paper has been to call attention to the changes which are taking place in physical anthropology. Under the influence of modern genetic theory, the field is changing from the form it assumed in the latter part of the nineteenth century into a part of modern science. The change is essentially one of emphasis. If traditional physical anthropology was 80 per cent measurement and 20 per cent concerned with heredity, process, and anatomy, in the new physical anthropology the proportions may be approximately reversed. I have stressed the impact of genetics on anthropology, but the process need not be all one way. If the form of the human face can be thoroughly

analyzed, this will open the way to the understanding of its development and the interpretation of abnormalities and malocclusion, and may lead to advances in genetics, anatomy, and medicine. Although evolution is fascinating in itself, the understanding of the functional anatomy which may be gained from it is of more than philosophical importance. The kind of systemic anatomy in which bones, muscles, ligaments, *etc.* are treated separately became obsolete with the publication of the "Origin of Species" in 1859. The anatomy of life, of integrated function, does not know the artificial boundaries which still govern the dissection of a corpse. The new physical anthropology has much to offer to anyone interested in the structure or evolution of man, but this is only a beginning. To build it, we must collaborate with social scientists, geneticists, anatomists, and paleontologists. We need new ideas, new methods, new workers. There is nothing we do today which will not be done better tomorrow.

Acknowledgements. My thinking with regard to the problems discussed has been influenced particularly by Wm. C. Boyd, S. R. Detwiler, Th. Dobzhansky, E. Mayr, H. H. Strandskov, P. A. Weiss, and S. Wright. The experiments described have been made possible by grants from the Viking Fund. The alizarin specimens were prepared by M. J. Baer. The split-line of the face was made by N. C. Tappen. The analysis of Neandertal man is being done by F. C. Howell.

DIVISION OF MYCOLOGY

PROGRESS OF RESEARCH RAMIFYING FROM THE FUNGI OF DETERIORATION*

By WILLIAM H. WESTON†

The importance of the deterioration of valuable matériel, such as textiles, equipment, instruments, and the other essentials for large-scale operations in the Pacific Theatre, focused attention on the necessity for extending our knowledge of the microbiological agents concerned. Necessarily, therefore, large collections of the organisms implicated in deterioration were assembled in various laboratories and, centering on these collections, there was intense activity of immediate contemporary work. After the war, some of these collections were no longer maintained, as the work on them had declined. Others, however, were taken over by agencies, such as the Quartermaster General Research Laboratories, and work on them continued actively. As a result, there has been a growing appreciation of these collections as valuable scientific material, for both applied and fundamental investigations, and they have been found to be not only of microbiologic interest and significance in themselves, but also of notable potentialities as favorable raw material especially suited for certain lines of research.

In the case of the collections which became the working material of the Quartermaster General Laboratories, the fungi concerned have proved to be of considerable interest. Certain aspects, such as their proportionate representation of taxonomic groups and the comparison of their components from diverse geographic localities in the Pacific region and the American tropics, have already been noted in published reports.

In their nature, these fungi do not make up a homogeneous, sharply defined, distinctive group. On the contrary, they have proved to be a heterogeneous assemblage comprising miscellaneous fungi, many of which are already better known in other capacities and associations. Their affiliations, alliances, and sources of origin show this heterogeneous character Their closest affiliation is with the mycological flora of the soil. In the extensive lists of soil fungi for various countries, practically all of them may be found. Among them are represented the active degraders of plant and animal material, especially the forms which break down cellulose and other prominent components of plants into the simpler stuffs that compose the soil. Among them also are some of the coprophilous fungi characteristically found on dung of herbivorous animals. Represented among them, too, will be found several that have been recorded from the macabre habitat of human bodies some months after burial in the soil. They also include many fungi which can make use of waxes, resins, gums, and oils, some which degrade keratin, and a few which give evidence of being able to utilize chitin. Fungi pathogenic to plants are well represented among them, and there are a few that are pathogenic to lower animals and a very few that have been implicated in attacking man.

In short, they comprise an assemblage of great diversity, and this very quality gives them value in their versatile utility toward the general program for the study and control of deterioration.

The suitability of these fungi as advantageous material for research has found expression along two lines. The first is the main line of investigation directed toward understanding, and thereby preventing, the multifarious activities of these fungi in the complex of deterioration. This is part of the original program for the Quartermaster

* This paper, illustrated with lantern slides, was presented at the meeting of the Division, April 27, 1951.

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General Research Laboratories, and, through their skillful and tenacious work, it has yielded results of notable fundamental as well as practical value.

The second line includes the ramifications of research on various aspects of the behavior of these fungi. It is here that certain of the activities of our laboratories at Harvard have centered, as a result of our work with some of these collections during the war. For their participation in these projects grateful acknowledgement is due my research associates and students, especially Mrs. Carol Buell Nash, Dr. Ruth Burke, Miss Elizabeth Weiant, George Eisenman, Hubert Caplan, Leo Kaplan, William Horwitz, David Marsden, and Peter Mazur. While the work has been diverse and not part of an integrated program, the investigations, for the most part, lie in the following areas: longterm survival under conservation conditions; organization and dominance in spores; interactions between various fungi; utilization of natural materials; and hardihood and versatility.

In the studies of survival, further work on conservation by submergence under mineral oil has established the survival of all but four of the original twenty-five representative test cultures after nearly six years, and has shown promising applications for the conservation of groups such as the aquatic fungi and certain algae in liquid media. Further work on conservation by the lyophil process has yielded some promising modifications and has led to other methods, such as spray-drying, which show some promise. Attempts to develop more sensitive, dependable, and rapid methods of evaluating the success of experimental lyophil procedures have shown bioassay by means of sensitive, highly organized spores to be impracticable, and chemical indicators, such as triphenyltetrazolium chloride, to be delicate but promising.

Organization and Dominance in Spores. This line of investigation grew out of our attempts to find some sensitive test organism that would serve as a quantitative indicator of the proportion or degree of survival in our studies of the effects of the lyophil process. Although it proved impracticable for such evaluation, it revealed features of fundamental interest in the organization and control shown in the germination of certain several-celled spores, such as those of *Pestalotia*, *Curvularia*, and *Helminthosporium*.

In the species of *Pestalotia* studied, the structural differentiation and the pattern and sequence of germination are notable. The spore comprises five cells: a stalk cell, thin-walled and hyaline, bearing the pedicel of attachment; a terminal cell, thin-walled, hyaline, bearing the three prong-like appendages; and, lying between these two hyaline cells, which do not germinate, three thick-walled, dense, dark-colored cells which are capable of germination. The basal one of these is of thinner wall, less dense content, and lighter color and swells more readily through absorbing water. Rapid germination in vigorous, uninjured spores is regularly (98+ per cent) from this cell only. After the spores have aged for three to six months, the percentage of germination decreases and the abnormal patterns of germination increase, being most commonly from the distal germinable cell and less frequently from the middle one. When, by precise puncturing or cutting with a micromanipulator, the basal germinable cell was killed, the distal germinable cell then germinated. When both basal and distal cells were killed, the middle one germinated. Once the germination of any cell had proceeded for more than five hours, however, no germination of the remaining germinable cells could be induced. The evidence, therefore, indicates that, under natural conditions, immediate germination regularly takes place from the more sensitive and responsive basal cell, the other two germinable cells remaining in reserve ready to germinate if the basal cell is incapacitated. Thus, obviously there is survival value in this neatly controlled mechanism.

Similarly significant, although slightly different, mechanisms were found in *Curbularia* and *Helminthosporium*, thus opening up an interesting line of investigation that is being extended to other comparable spores.

Interactions. Our two years of intensive work with the Quartermaster collection brought to light several instances of interaction, some being cases of mutualism or synergism, some of antagonism, and some of parasitism. A few were of such mycologic interest that they have been investigated further in our laboratories, and one or two might be noted here as representative examples.

One interesting case involved the advantageous reaction of a species of *Blakeslea* to a *Penicillium* contaminant. This *Blakeslea*, like most of the species of this beautiful tropical mold, could be induced only by special conditions to sporulate at all in culture, and then only rarely and sparsely. When cultured in the presence of a certain *Penicillium*, however, or when grown on media to which was added the filtrate from broth in which this *Penicillium* had developed, this Blakeslea produced abundant sporulation. Unfortunately, this interesting line of investigation was brought to an end when the culture of *Penicillium* was discarded in reducing the numerous duplicate cultures to a smaller representation.

Another case involved the parasitic invasion of the phycomycetous mold *Zygorhynchus* by a small *Septonema*-like, dematiaceous, imperfect fungus which had inconspicuously accompanied its host in the original supposedly pure culture derived from storage deterioration tests in Florida. This parasitism, which had attracted our attention on microscopic study of the culture, strikingly revealed itself to the mycologists at the Quartermaster General Laboratories while they were testing large numbers of cultures for utilization of cellulose. For this culture, contrary to the behavior of *Zygorhynchus* or any of its close relatives, showed active degradation of cellulose, and further investigation revealed that it was the inconspicuous parasite which was accomplishing this. Taking advantage of this difference, the two components of this association were isolated into separate pure cultures by growing the mixed culture on cotton cloth plus mineral nutrients for, since the *Zygorhynchus* did not develop, the rapidly extending *Septonema* was easily isolated. Further work showed that, in pure culture by itself, the parasite grew luxuriantly on a wide variety of nutrients with diverse nitrogen and carbon sources and, therefore, was not an obligate parasite. In mixed culture, the *Septonema* grew luxuriantly as an endoparasite on the *Zygorhynchus*, extensively invading its vegetative hyphae and the fundaments of the reproductive structures. Tests to determine the range of parasitism of the *Septonema* showed it to be actively parasitic on species of various representative genera of the sporangial *Mucorales* such as *Mucor*, *Rhizopus*, and *Circinella* and on *Cunninghamella*, *Syncephalastrum*, and various other representatives of the conidial series.

In contrast, on *Fusarium* and other imperfect fungi, it showed no parasitic growth, nor did it attack the mycelium of the several *Basidiomycetes* which were tested. Further work is being carried on to determine the extent of the parasitism of this mold to representatives of the various main groups of the *Phycomycetes*. In this, as in other cases in which parasitism and the accompanying phenomena of the susceptibility and resistance of various hosts are all reduced to the simplest possible terms, problems of significant potentialities are opened up.

Studies on the utilization of diverse natural materials have corroborated the fact that members of the phycomycetous order *Mucorales* do not utilize cellulose, while certain aquatic *Phycomycetes* do and that a wide range of Fungi Imperfecti which utilize relatively pure cellulose are not able to utilize cellulose from its intimate association with lignin in wood,

while various wood-rotting *Basidiomycetes* can do both.

Investigations of the hardihood and versatility of various fungi have shown that *Cladosporium herbarum* is notable for its tolerance of a wide range of temperature, of pH, and of chemicals and is responsible for a remarkable diversity of destructive activities.

In conclusion, it becomes apparent that the fungi which were collected because of their implication in the deterioration of important materials have proved to be of value not only by contributing to a better understanding of the processes of deterioration and the control thereof, but also by furnishing favorable working material for the investigation of various problems of biologic interest. In the first category, the mounting evidence of the effective work done by such agencies as the Quartermaster General Research Laboratories clearly ndicates the useful purpose which these ungi have been made to serve. Also, it should be noted that industry, for its part, already has taken advantage of the progress which has been made. As an example of this, it is noteworthy that precise and highly complicated electrical equipment, now being manufactured with discriminating use of critical materials invulnerable to mold attack and effective application of inert plastic finishes and fungicidal varnishes, successfully withstands test exposures to rigorous conditions of saturated humidity, optimum temperature, and highly destructive molds. This is in notable contrast to the vulnerability of such equipment produced during the earlier years of the last war, and is an encouraging indication of the general improvement resulting from better understanding of the nature and activities of the fungi of deterioration. Finally, the contribution of these fungi to the fundamental and academic aspects of research, although seemingly remote from any practical application, has considerable biologic interest.

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* In the January issue of TRANSACTIONS, Vol. 13, No. 3., Miss Hazel Overby was listed as being affiliated with Adelphi College. Miss Overby's correct affiliation is Hunter College, New York.